THE ADEQUACY, DIRECTION AND PRIORITIES FOR THE AMERICAN SCIENCE AND TECHNOLOGY EFFORT

HEARINGS

BEFORE THE

SUBCOMMITTEE ON
SCIENCE, RESEARCH AND TECHNOLOGY

OF THE

COMMITTEE ON
SCIENCE, SPACE, AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

ONE HUNDRED FIRST CONGRESS

FIRST SESSION

FEBRUARY 28; MARCH 1, 1989

[No. 7]

Printed for the use of the
Committee on Science, Space, and Technology

U.S. GOVERNMENT PRINTING OFFICE
97-468 ⇆ WASHINGTON : 1989

For sale by the Superintendent of Documents, Congressional Sales Office
U.S. Government Printing Office, Washington, DC 20402

H701-53

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THE ADEQUACY, DIRECTION AND PRIORITIES FOR THE AMERICAN SCIENCE AND TECHNOLOGY EFFORT

TUESDAY, FEBRUARY 28, 1989

U.S. HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY,
SUBCOMMITTEE ON SCIENCE, RESEARCH AND TECHNOLOGY,
Washington, D. C.

The subcommittee met at 9:37 a.m. in Room 2318 of the Rayburn House Office Building, the Honorable Dough Walgren, chairman of the subcommittee, presiding.

Mr. WALGREN. This morning we begin an exploration of the adequacy of Federal support for science and technology in the United States.

A particular emphasis of this hearing will be the process through which Federal R&D resources are allocated, and we hope that this discussion will include the advocacy of support for science and engineering education, which of course is so essential to provide the human resources to achieve national goals.

Recent discoveries in science have led to exciting research opportunities in a wide variety of fields. It seems, with regularity we read in the newspapers of the latest advances in genetic engineering and digital computers or new materials, and we also have seen a proliferation of big science and technology proposals such as the space station, superconducting supercollider, and even the aerospace plan, which would involve expenditures of very large amounts of money which could easily overwhelm the traditional amounts of R&D support which we have in our budgets.

At the same time that these new opportunities appear, we have ample evidence of serious deterioration in the research infrastructure of universities and have come to see the need to try to add strength to undergraduate education in science and engineering.

Clearly, the opportunities for new research and the need to rebuild the university-based research education in science and engineering both together exceed our resources.

We on this committee would argue that the payoffs for the Nation are so great that increased investments in science and technology are only prudent. However, even if we could double the science budget tomorrow, we would not escape the need to establish priorities, in view of the size of a number of these projects.

At present, we have no well-defined process for evaluating and prioritizing new and large-science initiatives, nor for systematically evaluating the balance of the overall Federal investment in research and development in the variety of fields that we try to serve.

The R&D budgets of the different Federal agencies are evaluated separately and largely independently, both within the executive branch and certainly here in the House and Senate. As a result, long-term planning and balancing of resources among fields and prioritizing seem to be not orderly tasks.

Over the two days of these hearings, we hope to identify effective ways for the Federal Government to evaluate the adequacy of funding support for science and technology to determine promising new research directions, and to establish priorities in our efforts.

Of particular interest are the criteria for evaluating competing research development projects in different fields and the organization arrangements for helping us to do a better job of allocating scarce resources.

Our first witness this morning is Dr. Robert White, President of the National Academy of Engineering, who is here representing the academie'sviews on setting budget priorities as developed in a recent report by the academy, entitled "Federal Science and Technology Budget Priorities: New Perspectives and New Procedures."

Our second panel is comprised of a distinguished group of senior scientists and philosophers related to science. We will have Dr. Lewis Branscomb, Director of the Science, Technology, and Public Policy Program of the Kennedy School of Government at Harvard; Dr. Cyert, President of Carnegie-Mellon University, from my own hometown of Pittsburgh; and Dr. Lawson Crowe, Professor of Philosophy at the University of Colorado.

Then, on the final panel we will have three, I suppose you might call them bench, but working—not to say that the others don't—

[Laughter.]

Mr. Walgren.—but working scientists who have made important research contributions in their scientific disciplines: Dr. Charles Fefferman, Professor of Mathematics at Princeton; Dr. Richard Muller, Professor and Faculty Senior Scientist at the Department of Physics at the Lawrence Berkeley Laboratory at the University of California; and Dr. Thomas Pollard, Professor and Director of the Department of Cell Biology and Anatomy at Johns Hopkins University.

We appreciate all of your coming and for the kind of direction you can give both the committee in the public discussion of this area. I know that all of you have lots of things competing for your time, and I especially appreciate your making, both today, and what goes into pulling together the views that you might offer to us, available to us as a committee.

With that, are there any other opening statements that members would like to make?

Sherry will have his when he comes, and we will recognize him at this point.

If not, let's start with Dr. White and we will get going.

Welcome to the committee. And let me say at the outset, as you know, having been here before, that any written detail and statement that you would like to make will be reproduced in the record as a matter of form, and you can certainly feel free to seize on points that you would like to call more attention to in the oral part of your presentation.

So, please proceed however you would like.

STATEMENT OF DR. ROBERT WHITE PRESIDENT NATIONAL ACADEMY OF ENGINEERING

Dr. WHITE. Mr. Chairman, members of the committee, I am pleased to be here to discuss the recommendations of the National Academy of Engineering together with the National Academy of Sciences and the Institute of Medicine, the recently published report entitled "Federal Science and Technology Budget Priorities," and I would like to submit the report for the record, Mr. Chairman, and I will just summarize the report's highlights.

Our report was in response to a request by the Congress contained in the Conference Report on the Concurrent Resolution on the Budget for Fiscal Year 1989.

The Conference Report asked the Academies and the Institute of Medicine to provide—and here I quote—"advice on developing an appropriate institutional framework and information base for conducting cross-program development and review of the Nation's research and development programs."

We were asked not to recommend specific program priorities. We make three types of recommendations, Mr. Chairman:

First, we propose a framework for guiding Federal science and technology budget preparation and assessment;

Second, we suggest a set of criteria which can be helpful in judging whether allocations for science resources are serving national objectives;

Third, we recommend changes in the executive branch process of preparing budgets and in the congressional process of reviewing Administration requests and appropriating funds.

I will briefly review each set of those recommendations.

First, a framework for assessing science and technology budgets: It is important to examine proposed expenditures for science and technology in relation to the many national purposes they serve. In our report we suggest four categories:

The first, science and technology activities supporting agency missions. Most agencies use science and technology to advance their ability to discharge their missions. These are important governmental functions serving the national welfare. The present budgetary processes traditionally do reasonably well in assessing science and technology budgets in this context.

Second, the maintenance and advancement of the Nation's science and technology base. Our economic growth, standard of living, provision of health care, an adequate defense and other social objectives are, in the long run, dependent on the talent and knowledge that comes from the national science and technology enterprise.

Many agencies, through their science and technology budgets, support this enterprise. It is a cross-agency responsibility.

But in the present budgetary scheme there is little attention paid as to how the total R&D expenditures are contributing to the education and training of students, for example, supporting the conduct of research, ensuring faculty adequacy, and providing for the requisite availability of facilities and equipment.

Third, the category which we would say involves the science and technology support for national priorities, as decided by the President and the Congress. Science and technology in many cases are central to achieving these priorities; for example, advancing our industrial competitiveness or achieving a quality environment. Normally, these priority goals require the contributions of many agencies. Such cross-agency efforts need to be addressed in a more consciously integrated manner.

We note in President Reagan's proposals, such cross-cutting analyses were made, for example, in the report, "Our Changing Planet: A U.S. Strategy for Global Change in Research." This is a start.

Fourth, major and costly science or engineering initiatives that require large incremental expenditures. From time to time scientific or technological opportunities arise which require very large expenditures. Examples are the superconducting supercollider, mapping and sequencing the human genome, and the space station. Projects of this sort will fall in one or more of the above three categories, and their priorities should be judged within these contexts. Because of their complexities, costs, and multiyear commitments, they impinge on the funding of other activities in these categories and, therefore, merit additional analysis.

In the report, we suggest illustrative, but not exhaustive, criteria in the form of questions that might be asked about activities in each of the categories to guide resource allocation decisions during the budget process.

Building on the current budget process. The current budget process provides for reasonable consideration of the contributions of agency science and technology programs to agency objectives. We do not propose that this process be changed.

Without proposing major changes in the basic budget process, we believe that there are useful changes which can be made in both the Executive and Congressional phases for those categories of purpose that involve the contributions of several agencies and span the jurisdictions of several congressional committees.

We propose that presidential budget guidance be provided to the agencies early in the budget cycle. Such guidance is absent in the present scheme of things. This will enable agencies to respond more effectively to both their mission's needs and the broader national priorities that the President has established. This guidance can then be used to judge agency proposals in relation to the President's priorities. Presumably the President's science and technology advisors and OMB would work together to prepare both the guidance and the assessments. The proposed council of science and technology advisors would be one way to involve the science and technology community. In addition, the science and technology advisor would be in contact with broad elements of the science and technology community throughout the year. For example, presidential guidance might emphasize support of graduate training in the physical sciences and engineering or advancement of generic manufacturing technologies or research into global climate change or the exploration of long-range application of high-temperature superconductors. This presidential guidance could accompany his submission to the Congress.

If the President's budget preparation and presentation follow these procedures, Congress will receive an explicit statement of the President's science and technology priorities. Budget allocations across agencies will be displayed. Congress will then have more information than it currently receives on which to evaluate its priorities.

For the process to be effective, Congress should also make complementary changes in its budget process. The Congress should devise arrangements that would enable it to consider the proposed science and technology budget allocations as they relate to the cross-cutting categories of purposes before the budget is disaggregated for consideration on an individual agency basis. It is, therefore, an appropriate function of the budget committees and the authorizing committees with broad science and technology oversight jurisdictions. The committees' views with recommended changes in the President's budget proposals would, hopefully, be communicated to the appropriations committees in a way that would provide them with the broader consequences of actions on individual agency budgets.

Now, I would like to comment on the national R&D investment. Finally, I want to bring to the committee's attention an issue that arose in our deliberations that confuses the true nature of our national R&D investment, which in the 1990 budget proposals exceeds $67 billion. It is the treatment of DoD R&D activities in our national R&D statistics. Combining the entire DoD R&D budget with the R&D activities of the civil agencies in an uncritical manner distorts the perception of the national science and technology effort. Basic and applied research, exploratory development, and perhaps some of the advanced development in DoD are similar to civilian agency science and technology activities. The great bulk of DoD R&D, however, is concerned with specific military equipment development, testing, evaluation, and prototype procurement, and are not comparable to other Federal science and technology activities. Using these assumptions, in looking at the FY 1990 budget, for example, the national R&D investment would be closer to $40 billion than $67 billion. We believe it would be desirable to clarify the extent of DoD R&D activities that are comparable to the civil R&D.

The recommendations I have highlighted and others in the report could help ensure that budgets for science and technology are treated in a manner that reflects their pervasiveness in meeting national objectives. We are pleased to note that the principles suggested in our report are already being introduced in the executive branch, as described in the "Special Analysis on Research and Development" of the Reagan budget and in "Building a Better America," submitted by President Bush.

I will be pleased to answer any questions, Mr. Chairman.

[The prepared statement of Dr. White follows.]

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TESTIMONY

ROBERT M. WHITE

PRESIDENT, NATIONAL ACADEMY OF ENGINEERING

BEFORE THE SUBCOMMITTEE ON SCIENCE, RESEARCH AND TECHNOLOGY
OF THE COMMITTEE ON SCIENCE, SPACE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES
February 28, 1989

2/27/89-DRAFT-BUDGTEST

TESTIMONY
ROBERT M. WHITE
PRESIDENT, NATIONAL ACADEMY OF ENGINEERING

BEFORE THE SUBCOMMITTEE ON SCIENCE, RESEARCH AND TECHNOLOGY
OF THE COMMITTEE ON SCIENCE, SPACE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES
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Our report was in response to a request of Congress contained in the Conference Report on the Concurrent Resolution on the Budget for FY 1989. The Conference Report asked the Academies and the Institute of Medicine to provide "advice on developing an appropriate institutional framework and information base for conducting cross-program development and review of the nation's research and development programs." We were asked not to recommend specific program priorities.

We make three types of recommendations. First, we propose a framework for guiding federal science and technology budget preparation and assessment. Second, we suggest a set of criteria which can be helpful in judging whether allocations for science and technology resources are serving national objectives. Third, we recommend changes in the Executive Branch process of preparing science and technology budgets and in the congressional process of reviewing Administration requests and appropriating funds. I will review briefly each set of recommendations.

A Framework for Assessing Science and Technology Budgets.

It is important to examine proposed expenditures for science and technology in relation to the many national purposes they serve. We suggest four categories.

1. Science and technology activities supporting agency missions. Most agencies use science and technology to advance their ability to discharge their missions. These are important governmental functions serving the national welfare. Present budgetary processes traditionally do reasonably well in assessing science and technology budgets in this context.

2. The maintenance and advancement of the nation's science and technology base. Our economic growth, standard of living, provision of health care, an adequate defense and other social objectives are, in the long run, dependent on the talent and knowledge that come from the national science and technology enterprise. Many agencies through their science and technology budgets support this

enterprise. It is a cross-agency responsibility. But in the present budgetary scheme there is little attention paid as to how total R&D expenditures are contributing to education and training of students, supporting the conduct of research, ensuring faculty adequacy, and providing for the requisite availability of facilities and equipment.

3. The science and technology support for national priorities as decided by the President and Congress. Science and technology in many cases are central to achieving these priorities; for example, advancing our industrial competitiveness or achieving a quality environment. Normally these priority goals require the contributions of many agencies. Such cross-agency efforts need to be addressed in a more conscious and integrated manner. We note that in President Reagan's budget proposals, several such crosscutting analyses have been made, for example in the report, Our Changing Planet: A U.S. Strategy for Global Change Research. This is a start.

4. Major and costly science or engineering initiatives that require large incremental expenditures. From time to time scientific or technological opportunities arise which require very large expenditures. Examples are the superconducting super collider, mapping and sequencing the human genome, and the space station. Projects of this sort will fall in one or more of the above three categories

and their priorities should be judged within those contexts. Because of their complexity, costs, and multiyear commitments, they impinge on the funding of other activities in these categories and therefore merit additional analysis.

In the report, we suggest illustrative but not exhaustive criteria in the form of questions that might be asked about activities in each of the categories to guide resource allocation decisions during the budget process.

Building on the Current Budget Process

The current budget process provides for reasonable consideration of the contributions of agency science and technology programs to agency objectives. We do not propose that this process be changed.

Without proposing major changes in the basic budget process, we believe there are useful changes that can be made in both the Executive and Congressional phases for those categories of purpose that involve the contributions of several agencies and span the jurisdictions of several congressional committees.

We propose that presidential S&T budget guidance be provided to agencies early in the budget cycle. Such guidance is absent in the present scheme of things. This will enable agencies to respond more effectively to both their mission needs and the broader national priorities that the President has established. This guidance can then be used to judge agency proposals in relation to the President's priorities. Presumably, the President's Science and Technology Advisor and OMB would work together to prepare both the guidance and the assessments. The proposed Council of Science and Technology Advisors would be one way to involve the science and technology community. In addition, the Science and Technology Advisor would be in contact with broad elements of the science and technology community throughout the year. For example, presidential guidance might emphasize support of graduate training in the physical sciences and engineering or advancement of generic manufacturing technologies or research into global climate change or the exploration of long-range applications of high-temperature superconductors. This presidential guidance could accompany his submission to Congress.

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For the process to be effective, Congress should also make complementary changes in its budget process. The Congress should devise arrangements that would enable it to consider the proposed science and technology budget allocations as they relate to the crosscutting categories of purposes before the budget is disaggregated for consideration on an individual agency basis. It is therefore an appropriate function of the budget committees and the authorizing committees with broad science and technology oversight jurisdictions. The committee's views with recommended changes in the President's budget proposals would hopefully be communicated to the appropriations committees in a way that will provide them with the broader consequences of actions on individual agency budgets.

The National R&D Investment

Finally, I want to bring to the Committee's attention an issue that arose in our deliberations that confuses the true nature of our national R&D investment, which in the 1990 budget proposals exceed $67.0 billion. It is the treatment of DOD R&D activities in our national R&D statistics. Combining the entire DOD R&D budget with the R&D activities of the civil agencies in an uncritical manner distorts the perception of the national science and technology effort. Basic and applied research, exploratory development, and perhaps some of advanced development in DOD are similar to civilian agency science and technology activities. The great bulk of DOD R&D, however, is concerned with specific military equipment development, testing, evaluation and prototype procurement, and are not comparable to other federal science and technology activities. Using these assumptions in looking at the FY 1990 budget, for example, the national R&D investment would be closer to $40.0 billion than $67.0 billion. We believe it would be desirable to clarify the extent of DOD R&D activities that are comparable to civil R&D.

The recommendations I have highlighted and others in our report could help ensure that budgets for science and technology are treated in a manner that reflects their pervasiveness in meeting national objectives. We are pleased to note that the principles suggested in our report are already being introduced in the Executive Branch as described in the "Special Analysis on Research and Development" of the Reagan budget and in "Building a Better America" submitted by President Bush.

I would be pleased to answer any questions. Thank you.

Federal Science and Technology Budget Priorities

New Perspectives and Procedures

NATIONAL ACADEMY OF SCIENCES
NATIONAL ACADEMY OF ENGINEERING
INSTITUTE OF MEDICINE

December 1988

FEDERAL S&T BUDGET PRIORITIES: NEW PERSPECTIVES AND PROCEDURES

Summary of Recommendations of the National Academy of Sciences, National Academy of Engineering, and the Institute of Medicine

Congress, the outgoing Administration, and President-elect Bush all expect the current rapid progress in science and technology to bring national and global benefits such as industrial productivity and competitiveness, effective measures against diseases such as AIDS, new materials to replace dwindling natural resources, military security, and environmental protection. But this promise is tempered by the reality of the federal budget deficit and the statutory requirement to eliminate it.

These conflicting pressures give rise to two concerns. First, is the federal government investing adequately for the long term in the training, research, and research infrastructure that make possible S&T advances with practical applications? Second, is the federal government deciding priorities among S&T opportunities in a way that will best advance the national interest?

A Framework for Assessing S&T Budgets. To help the Administration and Congress answer these questions, a committee of Councilors of the National Academies of Sciences and Engineering and the Institute of Medicine, convened at the request of the Senate and House Budget Commitees, recommends that S&T budget priorities be considered in four non-mutually exclusive categories:

- S&T activities in relation to sponsoring agencies' missions;
- the activities of several agencies that contribute to the S&T base of training, research, and its infrastructure;
- the activities, usually of several agencies, that contribute to broad national objectives such as industrial competitiveness or environmental protection; and
- activities that constitute major initiatives by virtue of their considerable cost over several years.

From beginning to end, the current budget process encourages quite thorough consideration of each agency's S&T programs in relation to its mission. With respect to activities in the three other categories that cut across agency, OMB, and congressional committee jurisdictions, however, the process of budget development and review is unsystematic and sometimes haphazard.

Procedures for Considering S&T Budgets. Without proposing major changes in the basic budget process, the Academies and the Institute recommend additional steps in the Executive Branch and congressional phases to ensure that S&T budgets are more in line with demonstrated needs and national priorities in these cross-cutting areas:

- Early in the budget cycle, the President's S&T adviser, working with the OMB director, should assist the President in developing a set of specific cross-cutting S&T objectives that are included in the initial budget instructions to the agencies and criteria for OMB review of agency proposals.
- The President's budget submission to Congress should contain a statement of these S&T priorities that guided the preparation of the budget.
- Before the President's budget is disaggregated for consideration by several authorization committees and appropriations subcommittees, the congressional budget and appropriations committees should conduct reviews of cross-cutting S&T activities and incorporate their judgments into recommendations for agency budget allocations.

The Academies and the Institute also support a 2-year budget cycle, especially for long-term federal investments in the nation's future.

Copies of the report, "Federal S&T Budget Priorities:
New Perspectives and Procedures," are available from
the Office of Government Affairs,
National Research Council,
2101 Constitution Avenue, N.W.,
Washington, D.C., 20418, or by calling 202/334-1601.

Federal Science and Technology Budget Priorities

New Perspectives and Procedures

A Report in Response to the Conference Report
on the Concurrent Resolution on the Budget
for Fiscal Year 1989 (H. Con. Res. 268)

NATIONAL ACADEMY OF SCIENCES
NATIONAL ACADEMY OF ENGINEERING
INSTITUTE OF MEDICINE

National Academy Press
Washington, D.C. 1988

National Academy Press 2101 Constitution Avenue, NW Washington, DC 20418

The National Academy of Sciences (NAS) is a private, self-perpetuating society of distinguished scholars in scientific and engineering research, dedicated to the furtherance of science and technology and their use for the general welfare. Under the authority of its congressional charter of 1863, the Academy has a working mandate that calls upon it to advise the federal government on scientific and technical matters. The Academy carries out this mandate primarily through the National Research Council, which it jointly administers with the National Academy of Engineering and the Institute of Medicine. Dr. Frank Press is President of the NAS.

The National Academy of Engineering (NAE) was established in 1964, under the charter of the NAS, as a parallel organization of distinguished engineers, autonomous in its administration and in the selection of members, sharing with the NAS its responsibilities for advising the federal government. Dr. Robert M. White is President of the NAE.

The Institute of Medicine (IOM) was chartered in 1970 by the NAS to enlist distinguished members of appropriate professions in the examination of policy matters pertaining to the health sciences and to the health of the public. In this, the Institute acts under both the Academy's 1863 congressional charter responsibility to be an adviser to the federal government and its own initiative in identifying issues of medical care, research, and education. Dr. Samuel O. Thier is President of the IOM.

Printed in the United States of America

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INTRODUCTION AND SUMMARY

This report is in response to a request of Congress contained in the Conference Report on the Concurrent Resolution on the Budget for FY 1989, adopted on June 6, 1988. The Conference Report asked the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine to provide

> ... advice on developing an appropriate institutional framework and information base for conducting cross-program development and review of the nation's research and development programs. This should be structured in such a way that it can be used by both the Executive Branch and Congress as a method for reviewing program contents and strategies and in determining funding and organizational priorities for science and technology (House Report 100-658 on H. Con. Res. 268).

The request originated with the Senate Committee on the Budget, whose report on the Senate version of the FY 1989 Budget Resolution expressed dissatisfaction with the lack of a "coordinated national research and development budget" and suitable categories and criteria for judging the allocation of resources to and among science and technology (S&T) activities. In asking the help of the Academies and the IOM, the Senate Budget Committee emphasized the importance of a scientific and technical perspective in developing a framework for setting priorities in federal research and development (see Appendix 1).

This report has been prepared by a committee composed of the Presidents of the two Academies and the Institute of Medicine and members of their respective institutional councils. A group of Academy staff members, many with federal budget experience in various settings, provided support for the project. In addition to the committee's deliberations, the study process included consultation with members of Congress; staff of congressional budget, appropriations, and authorization committees and support agencies; officials of the Executive Branch; and other people knowledgeable about the federal budget process and policymaking for science and technology.

In interpreting its charge, the committee at the outset drew two conclusions that determined the scope and focus of its review:

> First, the analysis was to include all science and technology activities directly supported by the federal government, including the education of scientists and engineers and the financing and operation of specialized facilities as well as the conduct of research—basic and applied—and development. "Research and development" is the category conventionally used in budget submissions and analyses of expenditures, but the commit-

tee judged it important to encompass activities that are instrumental to but not necessarily counted as part of R&D, such as education.

Second, the analysis was to consider how public officials think about, prepare, and review science and technology budgets year in and year out. During the most recent budget cycle especially there has been a great deal of discussion about priorities among current federal S&T programs and projects; but it was not feasible for, nor was it expected of, this committee to recommend what resource allocations ought to be made this year or next. Rather, we suggest an analytical perspective and changes in the budget process that will aid decisions about resources for science and technology over the long term.

We have two concerns: Are we investing adequately for the long term to sustain the enabling S&T infrastructure and are we deciding priorities among S&T opportunities in a way that will best advance the national interest?

We recommend that S&T budget priorities be considered in four non-mutually exclusive categories:

- the S&T activities of individual agencies in relation to each agency's mission;
- the aggregate contribution of several agencies to the science and technology base of the nation, including fundamental research, its supporting infrastructure, and its continued production of scientists and engineers;
- the contribution of S&T activities, frequently supported by several agencies, to national objectives that are given priority by the President and/or Congress (e.g., industrial competitiveness, environmental protection, and prevention and treatment of AIDS);
- a set of major S&T initiatives, with different purposes and character, that attract attention in any budget year primarily because of their cost and budgetary consequences for other S&T activities across agencies.

For science and technology activities in direct support of agency missions there is an established process of budget development and review, beginning with agency proposals taking into account any initial administration guidance, running through the Office of Management and Budget (OMB) examination and any agency appeals, and culminating in congressional authorizations and appropriations. In this process responsibilities are reasonably well defined and the results generally serve agency objectives. We do not propose that this process be changed.

With regard to activities in the three categories that cut across several agencies, however, the process is unsystematic and sometimes haphazard because responsibility is dispersed. Without proposing major changes in the basic budget process, we recommend additional steps in current Executive Branch and congressional procedures to ensure that S&T budgets are more reflective of demonstrated needs and national priorities in these cross-cutting areas.

- The President's objectives in the cross-cutting S&T areas should be included in initial OMB instructions to the agencies for the preparation of their budgets, and these priorities should guide the formulation of the President's final budget proposal to Congress.
- The President's budget submission to Congress should contain a statement of his priorities for the S&T base, S&T activities serving national objectives, and major S&T initiatives as well as a summary of agencies' mission-related S&T activities.
- The congressional budget and appropriations committees should conduct reviews of cross-cutting S&T activities and incorporate their judgments into recommendations for agency budget allocations.
- Following the enactment of necessary authorizations and appropriations, the Executive Office of the President should work with agency heads to see that S&T programs are carried out in accordance with the President's priorities as modified by Congress.

We believe that these recommendations will serve the nation well in a period of pressing resource constraints and growing expectations that science and technology will yield significant material benefits.

SCIENCE AND ENGINEERING IN FEDERAL BUDGETS

Recent federal budgets for S&T demonstrate that Congress and the Administration recognize the growing opportunities in S&T and their contribution to important national objectives. At the same time, policymakers in both branches are faced with a persistent large federal deficit and a statutory requirement to eliminate it. These conflicting pressures give rise to concerns about the federal budget process for S&T.

The unusual circumstances of the FY 1989 budget cycle in particular drew attention to S&T in the federal budget. The Bipartisan Budget Summit Agreement concluded in November 1987 permitted only a 2 percent increase (on the order of $3 billion) above the FY 1988 level of the nondefense discretionary portion of the federal budget that finances all civilian S&T programs along with housing, health, and other domestic programs apart from entitlements such as Social Security and Medicare. In his proposed FY 1989 budget, President Reagan asked Congress to allocate the equivalent of that increase to S&T,

including initiatives certain to entail large expenditures over several years—a Superconducting Super Collider (SSC) and a civilian manned Space Station. A doubling of the budget of the National Science Foundation over five years and a project to map and sequence the human genome also were proposed.

The widely anticipated crunch in funding for civilian programs did not occur in the FY 1989 appropriations to the degree many feared. Because of a change in the FY 1988 base, Congress was able to raise the $3 billion ceiling somewhat. It approved all the President's S&T initiatives, albeit at lower spending levels or with conditions intended to postpone some decisions for the incoming Administration. Overall, Congress approved $2 billion of the requested $3 billion increase for NASA, NSF, and research programs of DOE.

As for FY 1990 and beyond, there are signs that fiscal constraints will tighten further. The deficit reduction required to meet mandated levels continues to increase. Resistance to raising taxes, curbing entitlements, and cutting defense spending is strong. Interest payments on the federal debt continue to grow. And demands to address domestic social needs will be hard to deny.

The fact remains that in recent years, under growing pressure, the Administration and Congress have quite consistently supported S&T. Witness not only the FY 1989 appropriations but also the total federal investment in research and development, now exceeding $60 billion annually by the government's accounting. [For reasons enumerated in Appendix 2, we recommend for further study the current classification of Defense Department R&D that represents a major share of the total.]

This support for S&T is based on recognition that

- Investments in S&T are essential to address a growing list of national needs and objectives, including economic development, international competitiveness, and environmental protection as well as military security and public health.
- Expanding opportunities and breakthroughs in many areas of science and engineering are likely to yield significant economic and other benefits. Examples range from biotechnology to high-temperature superconductivity.
- Certain national emergencies require support for S&T investments aimed at solutions. The most obvious current example is the AIDS epidemic; others are the *Challenger* accident and its effect on the nation's space launch capacity and the precipitous loss of market share by the U.S. semiconductor industry.
- Investments in fundamental research and the advanced training of scientists and engineers are a precondition for reaping the practical benefits of S&T, and the federal government should make a major contribution to these investments.

These views will not necessarily prevail if fiscal constraints become much tighter, but budget outcomes to date are a measure of the strength of the political support for S&T. The federal budget process has not shortchanged S&T, nor has it failed to make choices when necessary.

Nevertheless, there are two issues: First, are we investing adequately for the long term to sustain the enabling S&T infrastructure? Second, are we deciding priorities among S&T opportunities in a way that will best advance the national interest? These would be compelling issues even if the budget climate were better. In a period of limited resources, there is an even greater premium on making the best-informed budget allocations possible.

In our review, we identified three types of S&T activities that should receive increased attention in the budget process in order to address these concerns. The activities are:

- *Investments in the S&T base—training, research, and its infrastructure.* These are required to sustain all scientific and engineering research and applications. The nation's scientific productivity, technological advancement, and commercialization capacity are directly dependent on the breadth and vigor of this base. These functions are supported through the budgets of several agencies.
- *S&T activities that contribute significantly to national economic, social, and political objectives pursued by the President or Congress or both.* Examples are industrial development and competitiveness, improvement in environmental quality, prevention and treatment of disease, and enhancement of national prestige. Typically, these objectives and the supporting S&T activities are shared responsibilities of several agencies.
- *New S&T initiatives, some but not all of them capital projects, that entail large expenditures.* These are often justified in terms of multiple benefits—new knowledge, jobs, national prestige, future economic growth, social welfare, and/or security. In a tight fiscal climate, such sharp step increases in the budget are difficult to accommodate except at the expense of other programs, including ongoing S&T activities.

The current budget process, which is designed primarily to consider how each agency's S&T programs contribute to its mission, will often inadequately address needs and define S&T objectives in these cross-cutting areas.

6

A FRAMEWORK FOR ASSESSING S&T BUDGETS

Overview

We propose a framework for guiding federal S&T budget preparation and assessment that provides a perspective on policy objectives and activities across as well as within agencies. The framework includes four nonmutually exclusive categories (see Table 1):

- S&T in the context of agency missions
- The S&T base
- S&T in relation to national objectives
- Major S&T initiatives

The framework will provide a basis for administration officials and congressional committees to sort out and make more explicit the characteristics of federal S&T programs and their contributions to the nation's goals and objectives. It will help identify what S&T opportunities and deficiencies need special attention, what fields or activities are not as productive or important as they once were, and what programs could be strengthened by collaboration and less duplication among the relevant agencies.

Agency Budgets and Missions

The starting point for assessing federal S&T budgets is a judgment about the mix, level, and quality of S&T in relation to other activities that will best advance the mission of a particular agency or department. The President's budget submission to Congress, including the "Special Analysis of Research and Development," is based on agency proposals as adjusted by OMB or the President. Congress, too, is organized to consider S&T in the context of an agency's entire portfolio of activities. With a few notable exceptions (e.g., the Department of Energy and the Environmental Protection Agency), one authorization committee and one appropriations subcommittee in each House has legislative and funding responsibility for an agency's operations.

The process by which the contributions of S&T programs to agency missions are judged and resources allocated works quite well to meet mission objectives ranging from advances in weapon system capabilities and health care to technical standards development and agricultural productivity. This is attributable in part to the fact that roles and responsibilities are well defined. It is also generally agreed that this pluralistic approach to S&T budgeting has contributed to the strength of the U.S. S&T system overall. Countries with highly

TABLE 1 Framework for Assessing S&T Budgets (Categories Are Not Mutually Exclusive)

Category	Characteristics	Examples
Agency budgets and missions	Agency S&T activities viewed in term of their contributions to individual agency goals and objectives	Nuclear and alternative energy R&D in DOE Submarine acoustics in DOD Cell biology in HHS Influence on learning in DOED Plant disease resistance in USDA Fundamental research in chemistry in NSF Standards development in NIST Aeronautical research in NASA
S&T base	Activities that provide the people knowledge, and infrastructure to carry out S&T Activities supported across many agencies and under the jurisdiction of several congressional committees	Basic and applied research programs in NSF, HHS, DOD, DOE, NASA, USDA, EPA, etc. Student fellowships in DOED, NSF, HHS, DOD, DOE, NASA, etc. Equipment and instrumentation programs in HHS, DOE, NSF, USDA, NASA, DOD, etc. Facilities for research, animal care, and growing and using special materials supported by NSF, DOD, HHS, DOE, NASA, etc. K-12 materials development in NSF, DOED, NASA, etc. Student internships in federal laboratories in DOE, NIH, etc.
S&T applied to national objectives (presidential and congressional priorities)	Stated priorities of President and Congress with major S&T components Frequently supported by several agencies and within the purview of several congressional committees	Understanding and ameliorating global change in EPA, DOE, NSF, NASA, USDA, NOAA, etc. Industrial development in biotechnology, superconductivity, manufacturing technologies in HHS, DOD, Commerce, NASA, NSF, DOE, USDA, etc. Alternative sources of energy in DOE, NSF, DOD, USDA, etc. AIDS in HHS, DOED, DOD, State Dept., etc. Creation of nuclear defense (Strategic Defense Initiative [SDI] in DOD) Increase capacity for exploration of space (Space Station in NASA)
Major S&T initiatives	Significant increase (and sometimes decreases) in budgets over several years Budgetary consequences across agencies Fall in one or more of above three categories	Superconducting Super Collider (SSC) Mapping and sequencing the human genome Space Station

centralized research systems have shown growing interest in emulating our coupling of S&T support and agency missions.

Science and Technology Base

The nation's commitment to sustaining a world class S&T capacity requires investment in the S&T base, which includes undergraduate training in science and engineering; the graduate education of scientists and engineers in a research setting; the conduct of fundamental and generic applied research on the frontiers of science and engineering, primarily in universities but also in federal, nonprofit, and corporate laboratories; and procurement and maintenance of equipment and facilities required for education and academic and federal laboratory research.

These activities are supported through the budgets of several agencies that are subject to review by different parts of OMB and various congressional committees. Support of the S&T base is central to the missions of NSF and HHS, but DOD, DOE, USDA, NASA, and other agencies make significant contributions.

Any scheme for examining federal S&T budgets must recognize that the S&T base is the bedrock of the nation's ability to use science and technology in the national interest and that it requires continual replenishment.

Continuity does not imply steady funding of the same activities and institutions through the same programs and agencies year after year. On the contrary, the enterprise ought to be highly dynamic. Policymakers must be able to respond flexibly to scientific breakthroughs that suddenly transform an area of research (e.g., high-temperature superconductivity), the invention of a powerful new instrument (e.g., gene-sequencing machine) or conceptions of new facilities that would aid research and training (e.g., supercomputer centers and networks), unexpected shortages of science and engineering personnel, or changing institutional relationships (e.g., the emergence of university-industry research partnerships). And as if that were not a sufficient challenge, budget makers and analysts must be attuned to differences among a wide range of fields. Some changes affect many disciplines, others only a part of a single discipline.

Sustained attention to the S&T base should be a part of the budget process in both the Executive and Legislative Branches of government. There need not be an annual review of every aspect of the base in every field, but each aspect should be reviewed periodically and there should be a capacity for considering a few key issues in any budget year.

S&T Applied to National Objectives (Presidential and Congressional Priorities)

At any time a limited number of national issues will be on the agendas of the President and Congress. Recent experience suggests that an increasing share of them will have prominent S&T components. Current and emerging examples are industrial development and international competitiveness, dependent on advancing S&T in such fields as superconductivity, microelectronics, materials, manufacturing technologies, and biotechnology; amelioration of global environmental changes; and prevention and treatment of AIDS.

Because these are broad issues, analysis, policy formulation, program implementation, *and* the support of relevant S&T frequently are dispersed among several agencies, as illustrated in part in Table 1. In theory the formulation of S&T budgets in these areas should entail review and judgments about the respective roles of the interested agencies, the relative contribution of S&T activities in each case, and the questions to be addressed by science and engineering. Although some reviews of this kind do take place, they are not done systematically. Congress recently expressed its desire to have such reviews carried out. In the 1988 Omnibus Trade and Competitiveness Act, the President is directed to submit with his FY 1990 budget cross-cutting analyses of federal efforts to advance four industrial technologies of major competitive significance—superconducting materials, semiconductor design and manufacturing, optoelectronics, and advanced manufacturing technologies.

Major S&T Initiatives

Finally, there are S&T activities or changes in programs that entail significant increases (or, less commonly, reductions) in agency S&T budgets. Recent examples include proposals to construct a SSC, deploy a manned Space Station, and map and sequence the human genome.

Projects of this sort will fall in one or more of the above three categories and should be evaluated accordingly. Because of their size and cost, however, they also impinge upon other activities in these categories, including activities beyond their sponsoring agencies, and therefore merit additional analysis as described in the next section. It is increasingly important that future outlays for all large projects receive systematic review.

Applying the Framework

The analysis of S&T budgets in terms of their contributions to *agency missions* is reasonably thorough. It is in the three categories of activities that cut across agency boundaries that greater attention needs to be brought to bear and new information developed in the course of the budget process. The framework described above helps to construct a set of questions or tests, the answers to which can help allocate S&T resources. The questions can be used in the initial development of presidential budget priorities conveyed to departments and agencies, and they can be applied to budget proposals from their inception in agencies through their review by OMB and the President and their consideration in Congress.

S&T Base

Analyses of activities cutting across agencies that contribute to the advancement of the S&T base usually should be carried out for a discipline or broad field (e.g., physical sciences, biomedical sciences, biological sciences, engineering, social and behavioral sciences). In a coordinated process, agency budgets and programs would be examined to determine their impact on needs for

- educating science and engineering personnel
- modernizing equipment and facilities
- supporting a mix of basic and applied research
- capitalizing on promising new research opportunities
- promoting interactions between related fields of science and engineering research
- distributing research support by geographic region and type of institution
- maintaining a mix of research modes: e.g., individual investigators, large groups, centers, university-industry partnerships
- balancing competitiveness and cooperation with research programs in other countries

S&T Applied to National Objectives

Analyses of cross-agency S&T activities as they contribute to presidential and congressional programs aimed at meeting social, economic, and other objectives need to be formulated in ways that relate to specific objectives, as shown in Table 1. In general, analyses should seek to determine the adequacy of S&T budgets with respect to

- achieving specific social, economic, or other objectives
- addressing the principal unresolved S&T questions related to the objectives
- achieving the necessary coordination across agencies
- providing for both near-term and long-term results

- complementing and capitalizing on nonfederal efforts, e.g., state and private company programs

Major S&T Initiatives

The analyses of special initiatives with large budget consequences should identify how the proposed program contributes to the three previous purposes, viz., mission agency objectives; the S&T base; and social, economic, and other objectives. The analyses also should consider impacts of the program on other S&T activities. In addition, analyses might document the extent to which special initiatives will

- pursue technical breakthroughs, e.g., biotechnology and superconductivity, or important new knowledge, e.g., the SSC and mapping and sequencing the human genome;
- entail large capital investments, e.g., the SSC and the Space Station, or a collection of many, smaller-scale projects, e.g., mapping and sequencing the human genome, or other arrangements;
- provide benefits such as development of new technologies (commercial, defense, areas related to other agency missions), education of scientists and engineers, creation of jobs, and contributions to U.S. scientific and technological leadership and other global interests.

Finally, for each initiative it is necessary to specify the institutions, individuals, and organizations that will be served; the costs; the opportunities for international cooperation and cost sharing; the management structure; and the timeliness of the program (why now rather than later?).

MODIFICATIONS OF THE EXECUTIVE AND CONGRESSIONAL BUDGET PROCESSES

Putting into practice the analytical framework outlined above requires adjustments in the Executive Branch process of preparing S&T budget proposals and in the congressional process of reviewing Administration requests and appropriating funds. We do not recommend radical changes in either context. We believe, however, that current processes can be modified to yield better results.

Executive Branch

The purpose of our recommendations is to ensure that Administration budget proposals reflect reviews not only of agency needs but also of the needs of the S&T base, the requirements of presidential and congressional policy initiatives that depend on S&T activities, and the objectives and impacts of major S&T initiatives. We propose that the Administration's development of the budget include two new features.

- Early in the budget cycle, the President should provide the agencies and departments with specific guidance on his S&T priorities in cross-cutting areas and on major S&T initiatives.
- Agency budget submissions should be developed, analyzed and adjusted in terms of this initial guidance and the questions posed in the preceding discussion of the framework.

We believe that the President's science and technology adviser, working closely with the director and professional staff of OMB, is best suited to coordinate both phases of this process. The additional procedures would not alter the traditional prerogatives of department secretaries and agency heads in formulating S&T programs and budgets in the context of agency missions, nor would they detract from the role of OMB in managing the preparation of the President's budget. On the other hand, our proposals would take advantage of the considerable scientific and technical expertise in both the agencies and OMB. They might require new coordinating mechanisms, although one already available under law is the Federal Coordinating Council for Science, Engineering, and Technology.

More specifically, the process might work as follows:

Being familiar with the President's general policy objectives and having consulted regularly with senior S&T and budget officials of the government, industrial officers, academic scientists and engineers, and professional societies, the science and technology adviser would develop recommendations on cross-cutting S&T priorities for the forthcoming fiscal year budget. With the President's modifications and endorsement, the recommendations would become part of OMB's initial instructions to the agencies. Examples of priorities that might be stated in these instructions are support of graduate training in the physical sciences and engineering, advancement of generic manufacturing technologies, research and monitoring to narrow the uncertainties regarding global climate change, and initial exploration of long-range applications of high-temperature superconductors. The President's statement of priorities would then become part of the criteria for evaluating agency budget submissions.

We also propose that the Administration submit to Congress with its budget recommendations a policy statement presenting the President's cross-cutting priorities for S&T that guided the preparation of his budget. It would include a summary of agencies' mission-related S&T activities as currently presented in the "Special Analysis of the Budget for R&D." The statement would also emphasize the Administration's proposals dealing with the S&T base, social, economic or other national objectives, or major new S&T initiatives.

Following enactment of necessary authorizations and appropriations, the President's science and technology adviser, director of OMB, and agency heads would work together to see that programs are carried out in accordance with the President's priorities as modified by Congress.

Table 2 shows the additional steps that we recommend alongside the sequence of steps now followed in the formulation of the President's budget.

Congress

If the President's budget presentation conforms to the recommendations above, Congress will have more information than it currently receives to evaluate the President's S&T priorities and budget allocations across agencies. Complementary adjustments in the congressional budget process could facilitate consideration of cross-cutting S&T programs and initiatives in the same manner as the recommended changes on the executive branch side. We make two procedural recommendations. We describe other options, suggested by members of Congress, that would affect consideration of S&T but arise from broad concerns about the effectiveness of Congress.

- Congress, no less than the administration, should consider programs and budgets in relation to the S&T base, national policy objectives, and major S&T budget initiatives as well as in the context of individual agency missions. This review should begin before the budget is disaggregated for consideration agency by agency and program by program. It is therefore an appropriate function of the budget committees of the Senate and House and of the authorizing committees with broad S&T oversight jurisdiction—the Senate Committee on Commerce, Science, and Transportation and the House Committee on Science, Space, and Technology.

  In each house a full committee or its special task force on S&T should examine the President's S&T budget submission; request whatever staff or support agency studies are needed to supplement analysis available from the Executive Branch; and take testimony from the President's science and technology adviser, agency officials, and knowledgeable people in the academic and industrial communities. We note that in recent years the Senate Budget Committee has conducted annual overview hearings on S&T in the federal budget. In the House, the Science, Space, and Technology Committee conducts annual S&T "posture" hearings.

TABLE 2 Current Practice and Proposed Additions to the Executive Branch Process for Preparing S&T Budgets

Current Practice	Proposed Additions
1. Focus is on how S&T activities meet the requirements of the missions of individual agencies	1. Add perspective of major initiatives and S&T activities that have impacts across several agencies
2. OMB manages preparation of President's budget	2. None
3. Cabinet secretaries and agency heads play key roles in formulating S&T programs and budgets to meet agency requirements	3. In addition, President's science and technology adviser, working with OMB, coordinates process to bring the cross-cutting perspective of S&T activities and major initiatives into the budget preparation by agency heads
4. OMB initiates process by sending agencies instructions for preparing their budgets	4. In addition to the types of instructions issued currently, the initial guidance includes the President's cross-cutting S&T priorities
5. OMB reviews and adjusts agency budget submissions and recommends final budgets to the President, with opportunities for agency appeals	5. The President's cross-cutting S&T priorities are used, along with other criteria, in reviewing agency budget submissions and recommending final decisions
6. President submits budget proposal to Congress; a special analysis on R&D is included that presents a summary of agency R&D budgets	6. The President's budget submission includes a statement of the S&T priorities that guided budget preparation, the rationale underlying them, and the resulting budget proposals

Whatever method of review is chosen, the budget committees should, in connection with the Budget Resolution, report their judgments on the President's priorities and recommend changes. The committees' recommendations would not dictate subsequent decisions in the congressional budget process; but they could influence allocations made within the appropriations committees, especially if the budget committees were to track the appropriations and report periodically on how they compare with the President's request and their own recommended adjustments.

- The appropriations committees also should have a procedure for reviewing cross-cutting S&T activities in light of the President's priorities and the budget committees' recommended adjustments. At a minimum, there should be discussions among the committee chairman and ranking minority member and the appropriate subcommittee chairs and their ranking members; and these discussions should begin before the appropriations committees allocate spending authority to their respective subcommittees (the 302b allocations).

Many members of Congress have proposed major changes of congressional budget procedure and committee structure that may receive serious consideration in a future Congress. One such procedural change is for the budget committees to propose, and both houses to approve as part of the Budget Resolution, the 302b allocations, which would then be binding on the appropriations committees. Proposed structural changes include placing on the budget committees more chairmen and ranking members of the appropriations subcommittees and authorization committees, creating a joint budget committee, consolidating the authorization and appropriations committees, and revising the jurisdictions of the appropriations subcommittees to combine similar programs and agencies.

We recognize that changes of this scope will only be adopted, if at all, out of broad concern for improving the performance of Congress in its legislative, budget, and oversight functions and after full consideration of the demands on members' time, their constituency and policy interests, and the distribution of influence among members and committees. But we urge Congress to consider also how procedural or structural changes would affect the review of S&T programs and budgets. In general, we favor a continuous process in which the early cross-cutting reviews of the President's S&T budget priorities would carry weight in the appropriations phase. If either house revises its committees' jurisdictions, we favor combining S&T programs with their parent agencies where possible. The budgets for S&T activities are now too large and their bearing on national affairs too great to be treated in an uncoordinated fashion.

Finally, there appears to be growing support for the government to shift from an annual to a two-year budget cycle. Indeed, the President's FY 1989 budget proposes two-year appropriations for a number of agencies. Although we have not considered how this should be accomplished in practice, we support biennial budgeting in principle. It could benefit longer-term federal investments such as S&T expenditures by enabling program managers to plan their S&T activities over a longer period, by giving S&T performers somewhat greater assurance of stable funding, and by encouraging decisionmakers to be more realistic about out-year spending requirements especially for major projects. A two-year cycle might also make it easier to accomplish the additional steps we propose in the development and review of the President's S&T budgets.

In recent Congresses, there has been a movement toward multiyear authorizations for agencies and programs with S&T components. If Congress decides to experiment with biennial appropriations, we believe that S&T programs should be leading candidates for such a trial.

CONCLUSION

In preparing and evaluating federal budgets for S&T, it is time to institutionalize a cross-agency perspective on three categories of activities: those contributing to the S&T base, those contributing to broader national policy objectives identified by the President and/or Congress, and those representing major new initiatives. These are activities undertaken by several agencies or with budget consequences for several agencies. Political support for such activities continues to grow, but none of the categories receives adequate attention in a budget process designed to consider S&T programs almost exclusively in relation to the mission of the parent agencies.

The Academies and the IOM examined several approaches to conducting cross-agency reviews of S&T programs in the budget process. We recommend an expanded framework for assessing and adjusting S&T programs across all agencies and procedural changes to apply the framework in both the Executive and Legislative Branches. The analytical framework should be useful immediately to officials involved in the budget process. The procedural changes can and should be implemented beginning with the FY 1991 budget cycle.

The National Academies of Sciences and Engineering and the Institute of Medicine hope to remain active participants in efforts to improve federal budgeting for S&T and will continue to promote discussion of these issues within the science and engineering community.

APPENDIX 1

Excerpt from the Report of the Committee on the Budget, U.S. Senate, on S. Con. Res. 113, Concurrent Resolution on the Budget FY 1989 (Sen. Report. 100-311)

Science and technology programs are widely recognized as being key to the nation's response to the challenge of economic competitiveness, as well as other major national priorities, such as health, environment, national defense, and quality of life.

Funding for these programs is spread across more than thirty independent federal agencies, making it difficult to develop a coordinated national research and development budget. Annual analyses performed by the Office of Management and Budget and the National Science Foundation merely compile agency proposals and are retrospective in nature. They generally are not used in the Executive branch decision-making process.

The Committee held hearings this year on science and technology. Much of the discussion centered on the problems and lost opportunities associated with the lack of coordination and the need for a better way to examine funding and organizational priorities in the federal effort in science and technology.

In light of these hearings and other findings, the Committee believes that the nation needs to develop a comprehensive set of mission categories to examine the allocation of resources to science and technology and identify gaps in funding. Any such approach should include substantial involvement from the scientific community in establishing priorities within the federal effort in research and development.

To undertake this effort, the Committee believes that the National Academy of Sciences, acting jointly with the National Academy of Engineering and the Institute of Medicine, should provide advice on developing an appropriate institutional framework and information base for conducting cross-program development and review of the nation's research and development programs. This should be structured in such a way that it can be used by both the Executive branch and Congress as a method for reviewing program contents and strategies and in determining funding and organizational priorities for science and technology. The Academies' advice should be provided to the appropriate Committees of Congress not later than November 15, 1988.

APPENDIX 2

Recommendation to Study Classification of DOD Activities in Federal R&D Statistics

Historically, DOD has accounted for a majority of federal R&D spending—currently, nearly two-thirds, or approximately $39 billion. Of this amount, between $4.5 billion and $5.5 billion are for activities similar to civilian agency S&T activities—basic and applied research and exploratory development. More than $30 billion are for advanced technology and military equipment development, testing and evaluation, and the operation of facilities for these specialized purposes—activities that are critical to the DOD but not comparable to other federal S&T activities. In the military budget buildup since the late 1970s, moreover, the latter budget categories have grown more rapidly than the former ones.

Lumping together as R&D the defense and civilian agency activities now classified as such may lead to overstating the national S&T effort overall and in certain research fields. We therefore urge the new Administration to assemble a group of experts to examine what DOD activities are appropriately classified as R&D. The group should include people with experience in OMB, the Office of Science and Technology Policy (OSTP), and DOD R&D management.

COMMITTEE OF COUNCILORS

PURNELL W. CHOPPIN is President of Howard Hughes Medical Institute. Dr. Choppin was a member of the faculty of the Rockefeller University for twenty-eight years, serving as Vice-President, Academic Programs from 1983 to 1985. He has served on numerous advisory committees of the National Institutes of Health.

KAREN DAVIS is Chairman of the Department of Health Policy and Management, School of Hygiene and Public Health, and Professor of Economics, Johns Hopkins University. An economist, Dr. Davis was earlier on the faculty of Rice University. She served for one year as Administrator, Health Resources Administration, Public Health Service, Department of Health and Human Services; and for three years as Deputy Assistant Secretary for Planning and Evaluation. She currently is a member of the Physician Payment Review Commission.

GERALD P. DINNEEN has been Vice-President for Science and Technology, Honeywell, Inc., since 1981 and held Department of Defense positions from 1977 to 1981 as Assistant Secretary of Defense, Communications, Command and Control, and Intelligence, and Principal Deputy Under Secretary of Defense, Research and Engineering. For twenty-five years he held various positions at Lincoln Laboratory, Massachusetts Institute of Technology. Dr. Dinneen is Foreign Secretary of the National Academy of Engineering.

JAMES D. EBERT, Director, Chesapeake Bay Institute, Baltimore, Maryland, is Vice-President of the National Academy of Sciences and immediate Past President of the Carnegie Institution of Washington. Dr. Ebert served on the faculty of the Johns Hopkins University from 1956 to 1978. He has been associated with a number of national laboratories, including the Brookhaven National Laboratory, the Marine Biological Laboratory, and the National Institutes of Health. He currently chairs the Government-University-Industry Research Roundtable.

ALEXANDER H. FLAX, Home Secretary of the National Academy of Engineering, was for six years Assistant Secretary for Research and Development, Department of Air Force and has twenty years experience with the Institute for Defense Analyses, serving as its President from 1969 to 1983 and President Emeritus from 1983 to the present. Dr. Flax's recent affiliations include the NATO Advisory Group for Aerospace Research and Development and the Defense Science Board.

HARRY B. GRAY is Arnold O. Beckman Professor of Chemistry at the California Institute of Technology where he joined the faculty in 1966, chairing the Division of Chemistry and Chemical Engineering from 1978 to 1984. Prior to his current position, he was on the Columbia University faculty for five years. Dr. Gray has served as Chairman of the NSF Advisory Committee on Chemistry and in advisory roles for the Oak Ridge National Laboratory, the Los Alamos National Laboratory and the Solar Energy Research Institute.

JOHN L. McLUCAS, a corporate director, scientist, and government administrator, is currently Chairman of QuesTech, Inc. Dr. McLucas was Executive Vice-President of COMSAT and President of its two major subsidiaries for ten years, and earlier was President and Chief Executive Officer of The Mitre Corporation. In the federal government, Dr. McLucas was Administrator of the Federal Aviation Administration, Secretary and Under Secretary of the U.S. Air Force for six years, Assistant Secretary General for Scientific Affairs, NATO, and Deputy Director of Defense Research and Engineering. He has served on advisory committees for NASA and the General Accounting Office.

RICHARD A. MERRILL, Dean of the University of Virginia School of Law from 1980 to 1988, and Daniel Caplin Professor, is currently on sabbatical as a Visiting Fellow of the National Wildlife Federation. Mr. Merrill was formerly Chief Counsel of the Food and Drug Administration. He has been a consultant to the Office of Technology Assessment, Food and Drug Law Institute, and the Environmental Protection Agency.

FRANK PRESS, President of the National Academy of Sciences since 1981, and President's Science Adviser during the Carter Administration, was reelected by the NAS membership to a second six-year term in 1987. As NAS President, Dr. Press serves concurrently as Chairman of the National Research Council. A geophysicist, he has served on the faculties of Columbia University, California Institute of Technology, and Massachusetts Institute of Technology. He served on the President's Science Advisory Committee during the Kennedy Administration and on the Baker and Ramo Presidential Advisory Committee during the Ford Administration. He was appointed by President Nixon to the National Science Board of the National Science Foundation and also served on the Lunar and Planetary Missions Board of the National Aeronautics and Space Administration.

PETER H. RAVEN is Director, Missouri Botanical Garden, a position he has held since 1971 while concurrently serving as Engelmann Professor of Botany, at Washington University, St. Louis. Dr. Raven has also been associated with the faculties of Stanford University, St. Louis University, and the University of

Missouri. Current activities include the position of Home Secretary of the National Academy of Sciences and Chairman of the NSF Advisory Committee for Biological, Behavioral, and Social Sciences.

H. GUYFORD STEVER, a corporate director, scientist, and engineer, served as White House Science and Technology Adviser to President Ford and Director of the Office of Science and Technology Policy and was Director of the National Science Foundation from 1973 to 1976. Prior to his government service, he was President of Carnegie-Mellon University from 1965 to 1972 and Professor of Aeronautics and Astronautics at the Massachusetts Institute of Technology for twenty years. Dr. Stever has served as NAE Foreign Secretary and as chairman of the National Research Council Committee on Space and the Panel on Technical Evaluation of NASA's Proposed Redesign of the Space Shuttle Solid Rocket Booster.

SAMUEL O. THIER is President of the Institute of Medicine, National Academy of Sciences, appointed to this position in 1985. He also currently is Visiting Professor of Medicine, The Johns Hopkins University School of Medicine, and Clinical Professor of Medicine at George Washington University School of Medicine and Health Sciences. Prior to his election as IOM President, he was Sterling Professor and Chairman of the Department of Internal Medicine, Yale University School of Medicine. Dr. Thier served on the faculties of Harvard and the University of Pennsylvania School of Medicine prior to his appointment at Yale. He did research at the National Institutes of Health from 1962 to 1964, and from 1980 to 1984 he served on the Director's Advisory Committee there.

ROBERT M. WHITE, President of the National Academy of Engineering since 1983, serves concurrently as Vice-Chairman of the National Research Council. Dr. White was the first Administrator of the National Oceanic and Atmospheric Administration. He was appointed Chief of the U.S. Weather Bureau by President Kennedy in 1964 and served under Presidents Kennedy, Johnson, Nixon, and Ford. He was appointed by President Carter to the National Advisory Committee on Oceans and Atmosphere. From 1977 to 1980, Dr. White was President of Joint Oceanographic Institutions, Inc., a consortium of universities engaged in deepsea drilling. From 1980 to 1983, Dr. White was President of the University Corporation for Atmospheric Research, a consortium of 50 universities with research programs in atmospheric sciences and technology.

ACADEMY STAFF GROUP

DAVID L. BODDE is Executive Director of the Commission on Engineering and Technical Systems. Prior to joining the National Research Council, Dr. Bodde was Assistant Director of the Congressional Budget Office and a Deputy Assistant Secretary of the Department of Energy. A graduate of the U.S. Military Academy, his graduate education includes nuclear engineering at MIT and business and economics at the Harvard Business School.

RAPHAEL G. KASPER, Executive Director, Commission on Physical Sciences, Mathematics, and Resources, was trained as a nuclear physicist. Dr. Kasper has served as a senior policy analyst in the Office of Science and Technology Policy, and as director of studies of the Nuclear Safety Oversight Committee in the Executive Office of the President.

LAWRENCE E. McCRAY, Executive Director, Committee on Science, Engineering, and Public Policy, received his doctorate from MIT in the field of science policy studies. In addition to his work on the effects of the Gramm-Rudman-Hollings Act and other assignments in science policy at the National Research Council, Dr. McCray has worked at the Office of Management and Budget and elsewhere in the Executive Office of the President.

STEPHEN A. MERRILL, Associate Project Director, is Director of the Office of Government Affairs of the National Research Council. He has participated in several Academy studies of science policy and technology and international competition. Dr. Merrill has served on various congressional staffs, most recently that of the Senate Committee on Commerce, Science, and Transportation. He was a member of the Senate staff group that developed the Senate committee reorganization plan adopted in 1977.

NORMAN METZGER is Deputy Executive Officer of the National Research Council and has been responsible for a number of examinations of science and its support. These include guidelines for the science and technology centers of the National Science Foundation; a series of reports on the outlook for science and technology; and a workshop on federal research and development support, emphasizing the relative costs and benefits of that support.

DON I. PHILLIPS, Project Director, is Executive Director of the Government-University-Industry Research Roundtable. Dr. Phillips prepared reports on federal and industry R&D budgets at the American Association for the Advancement of Science, served as a special science adviser in the Office of the Governor of North Carolina, and was the Director of the North Carolina Biotechnology Center.

PHILIP M. SMITH, Executive Officer of the National Research Council and National Academy of Sciences, has had broad science policy experience in the federal government. Former positions include Executive Assistant to the Director, National Science Foundation, and to the Science Adviser to the President; and Associate Director, Office of Science and Technology Policy, Executive Office of the President. He was Chief of the General Science Branch of the Office of Managment and Budget and has directed polar research programs of the National Science Foundation.

AUDREY G. WARD, Associate Director of Government Affairs, National Research Council, has been involved in legislative research and congressional liaison in the science policy area for the past 13 years. Earlier experience includes positions with the Bureau of the Census and as an intelligence analyst with the Central Intelligence Agency.

Mr. **Walgren**. Thank you very much, Dr. White. We appreciate that statement.

Just sort of as an explanation, maybe I should ask you, could you elaborate a little bit on what the executive branch is doing to introduce these cross-cutting processes?

Certainly we have a lot to learn from them. I am thinking how difficult that is in the Congress, because we have our different perspectives and sort of an ethic that no one individual's judgment rises above any other representative's judgment in validity and sincerity. And it makes it difficult to resolve differences under those terms.

But tell us a little bit about what the executive is doing.

Dr. **White**. Perhaps I can illustrate some of the things we have seen and illustrate it in terms of this document which was prepared, called "Our Changing Planet: A U.S. Strategy for Global Change Research." It is quite clear that this is a presidential priority. He has indicated that he is deeply concerned about environmental problems, and to undertake the research involved in studying global change requires the participation of many different agencies.

Here is a document that has been prepared in the Earth Sciences Committee, of the Federal Council of Science, Engineering, and Technology. And it displays for us the rationale, groups the activities into a small number of categories, like six categories of scientific research, and displays for us what each agency proposes to do, what responsibilities it proposes to undertake, and the amounts of money that are included in the President's budget to undertake this work.

It is a good example of a cross-cutting analysis of a major presidential, and obviously a congressional, priority area in looking at questions of global issues.

Mr. **Walgren**. It is our understanding that is a relatively unique effort on their part, that there are not many examples of cross-cutting within the executive agency.

And I couldn't help, in listening to your statement where you were saying, on page five, that "the presidential guidance might emphasize support for graduate training or advancement for generic manufacturing or the exploration of long-range applications of superconductors."

Our problem is that at least at first blush, listening to the President's address to Congress, the problem was that he emphasized one project and another project and another project, all of which were these megabuck projects that are, in a sense, mutually exclusive.

What do we do then?

Dr. **White**. Mr. Chairman, I did not mean to imply that our recommendations about looking at cross-cutting programs have been fully—I said we have just made a start. There is in the special analysis some other examples, not as elaborate as the one on "Our Changing Planet," on other cross-cutting areas.

We are pleased that a start has been made, but we would also like to emphasize this is only a start. It needs to be broadened and address the kinds of questions that we have outlined in our report and my statement.

Mr. WALGREN. Let me ask you in the context of these several categories of science efforts, (agency support, base support, national priority support,) where does little versus big fit in? Is there a value system that you feel we should be emphasizing? Inasmuch as our country has gained so much out of diversity, we believe historically—should there be a special emphasis on little science because of the inherent value of diversity and the individual researcher and the freedom that that represents?

Dr. WHITE. Well, I think posing the argument of little versus big is really the wrong way to pose the argument. The reason why I say that, technically Mr. Chairman, is that many big projects serve the interests of many individual investigators. For example, a large astronomical facility will serve the interests of individual astronomical researchers.

What you need to ask is whether there is enough money going to individual researchers who are not associated with and do not need access to large-scale facilities. And of course one must protect the ability to see funds going into the hands of individual investigators. But I think posing the question as little versus big may be the wrong way to pose the question.

And we need balance. You cannot investigate problems in the atmosphere without having access to a large aircraft. If you are going to do atmospheric chemistry, each investigator cannot have a large aircraft nor can each investigator have an SSC, and if you are going to map and sequence the human genome, it has to be done on a large megaproject basis. But its utility will be to individual investigators, not only across this country but across the world.

Mr. WALGREN. Maybe it would be wrong to say little versus big, but what about regular versus all-occupying where the concern is, as I understand it, that when you get into the billion-dollar individual projects like the supercollider at $4 billion. Maybe it is no longer a question of little versus big, but big versus behemoth or behemoth versus everything else?

Dr. WHITE. I don't mean to imply that we do not have a problem with these megaprojects. There is. And as our report states and as my statement points out, there is a problem with them because they are so costly and because you do deal with finite budgets. And hence expenditures for very large projects in fact, not only that particular project but impact lots of other things in the particular field and even outside that field. And we recognize that as a serious problem, as the Congress has recognized it as a serious problem.

Trying to judge the priority between a space station and mapping and charting the human genome might not be the right framework to judge priorities. It would seem to me that you have to judge whether the country will go ahead with the space station within the framework of where this country wants to be in space.

If indeed this country sees itself, and wants to see itself, for economic or political reasons as the preeminent space power in this world and if in achieving that objective it decides it wants to have human exploration of space, then it becomes logical that you fund a space station because you cannot do it otherwise.

That gets to be a decision which is not necessarily or entirely a scientific and technical decision. That gets to be a decision of where this country wants to be in terms of its relationships with other Nations and so forth.

Mr. **Walgren**. I don't mean to—I would like to move on to the other members, but should a decision like that be stripped out and recognized as a political decision rather than a science decision and, therefore, be targeted for funds that are reserved for or certainly not drawn from the science base?

Dr. **White**. That is certainly an option as to where you get the funding from. You may decide politically that you are not going to take it from the science base. All right.

Mr. **Walgren**. The question is why we would decide that if we say that the reason we were doing something is because not that we are that interested in the science but because we want the vision of it, we want the country to be first in space, for political purposes.

Then it would seem that that decision should be funded from some revenue source other than the science base, because it is not just defined on science at that point, given this hypothesis that the reason we are doing it is something other than the science.

Dr. **White**. I do not mean to imply that there are not important and useful scientific things that can be done with the space station and the human exploration of space. What I am saying is that the weight of the decision as to whether you do that or not has to be heavily weighted by your view of where this country wants to be in space.

Mr. **Walgren**. Let me turn to our ranking minority member for an opening statement, which will be included in the record, and then after that statement I would recognize Mr. Hayes for his questions.

Mr. **Boehlert**. Thank you, Mr. Chairman. I apologize for being a little bit delinquent this morning. My plane was late. That is always one of the problems we face.

This morning's hearing brought to mind an old "Peanuts" cartoon in which Charlie Brown walks dejectedly out to the doghouse during a storm and announces to Snoopy and Woodstock, "I did what you asked. I called the ASPCA, but they said they don't have enough money to give a free raincoat to every dog and bird in town."

Snoopy rolls his eyes and laments, "Every time I come up with a good idea, somebody has to bring up the budget."

That is the sentiment with which I am sure scientists can identify. But we in Congress must continue in the baleful role of Charlie Brown, spreading the bad news that there isn't enough money to buy a collider or a telescope or even a beaker for every scientist in town.

The open question is how to choose. There must be a better method than the current one for which the term "ad hoc" is probably too charitable.

The Administration acts like a promiscuous teenager willing to father just about any program, while Congress plays the role of the wayward-adoption agency farming out proposals to subcommittees willy-nilly like so many findlings, leaving them to fend for themselves in a new, indifferent environment.

Not surprisingly, no one is satisfied with the results. The recent report of the national academies on setting priorities was one attempt to introduce logic into the current procedure. The report is unexceptional as far as it goes. It failed, however, to address one crucial question: What criteria should we use in judging the relative merit of scientific proposals?

I don't think we should throw up our hands and say no such methodology exists. Budget writers regularly choose between funding such apples and oranges as welfare and defense. It should be possible to choose among science projects which, after all, are just different varieties of apples.

I am delighted that some of our witnesses today and tomorrow do propose specific procedures for setting science priorities. While I am not yet ready to endorse any particular method, especially one that assigns precise numerical scores, I think witnesses like Dr. Crowe are on the right track. Many a philosopher has asserted that a defining characteristic of human beings is free choice. Setting science policy would be an ideal opportunity to exercise it. The future of our scientific enterprise and indeed of our economy depend on it.

Thank you, Mr. Chairman.

Mr. WALGREN. Thank you, Mr. Boehlert.

The Chair recognizes the gentleman from Louisiana, Mr. Hayes.

Mr. HAYES. Ed earlier made a comment about the separation of R&D as it appears in defense budgets and as it appears otherwise in what would be more generally thought of as science and technology with, I assume, the implication of transferability being great. That would reveal by comparison to other leading industrial Nations. I think a percentage of budget that would show this country lagging rather substantially behind most of the industrial Nations.

What I might inquire from you is whether there is a way to further subdivide within the defense budget, so that perhaps we can see what parts should be separated as totally insusceptible to later transfer of technology and what part in fact somewhere down the pipeline leads to access in other sectors of the scientific community.

By doing so, we might learn whether we are going at the appropriate speed in allowing some of our business sector, the industrial applications of science that is no longer completely sensitive for security reasons, and where transfers are already taking place in marketplaces throughout the rest of the globe.

If the question makes any sense, then it may give us an insight into the mechanism and an internal improvement in DoD policy regarding transfer. If it makes no sense, then I am not sure a separation of DoD and non-DoD makes much sense either.

Dr. WHITE. In our report we suggest that it would be desirable to have a review of categorization of the DoD R&D budget. The reason why this is of concern relates to the points that you are making. We make judgments about our total national investment in R&D. And that that judgment gets distorted, in our view, by this very large number when we compare our investments in R&D nationally with the investments being made in other countries—in part, because of the question about the transferability of some of the things that are being done in DoD into the commercial sector. But that is not all of it. A lot of it is just not R&D. You know, going out with a prototype procurement or things of this nature is just not R&D, almost in any classification. That just happens to be the way DoD has done it, and it has served a good purpose in the DoD.

But if we are going to begin to make policy judgments about the adequacy of our total national R&D investment, then we are looking at distorted numbers, is what I am saying.

Mr. HAYES. I see. Thank you.

Mr. WALGREN. Thank you, Mr. Hayes.

The Chair recognizes the gentleman from California, Mr. Campbell. Welcome.

Mr. CAMPBELL. Thank you, Mr. Chairman.

Dr. White, I appreciate your testimony and the report.

I have just one question which deals with the funding of the project of some interest. And that is the Semitech consortium for semiconductor technology.

Now, as you know, the funding, the governmental source of that funding eventually included components from the Department of Defense. And it wasn't exactly decided that that would be the best course. In an abstract way, it just seemed to work out that way.

I ask this question, and I would like for you to comment, if you would, as to how defense involvement in funding for basic research of that kind as a customer is viewed by the academy as a positive or as a negative? And I would have a follow-up question as to what we might see in the HDTV area. But if you would be able to comment on Semitech in particular, I would be grateful, sir.

Dr. WHITE. Let me comment as an individual and not on behalf of the academies in this particular case, because I have expressed some of my own views on this question and those are the ones that I am prepared to give now.

I think Sematech represents an example of where we have, because of the absence of a national technology policy and because of the absence of a technology structure in the civilian part of the United States Government, have had to—the Department of Defense have had to fund things that may be very important in terms of supporting the civil technology base in this country. So, I think we need to be grateful that the DoD has taken up the cudgels in this particular case.

I think in the long run that is probably not where we should be heading as a country. Where we should be heading as a country is having a technology structure on the civil side of our Government that can deal not on an ad hoc basis, a willy-nilly basis as we have done today, but a long-term, systematic basis, taking responsibility for the strength and adequacy of our civil technology base. We don't have that today.

On the science side we do have that. We have structures in NIH, the National Science Foundation. We all agree philosophically that it is the responsibility of the United States Government to support basic science. We all agree with that. And we need vigorous basic science establishment in this country. We have one. Thank goodness we have one.

But we don't have a comparable thing in the technology area. The question is how to do that without intruding Government—without Government—into areas which are properly and logically the responsibility of the private sector. We have not been able to work that out. We had ideological problems that represent obstacles to working that out.

In my own view that is what we have to work out, and the time is long overdue when we devise some means of dealing with the adequacy of our civilian technology base.

Having said that, the Defense Department is a very large supporter of basic science in this country. I believe in the 1990 budget, the budget now before you, about $1 billion is now scheduled for what they call the 6.1 category in the Department of Defense. That is the basic science budget. It is a very important allocation of funds for the support of basic science in this country. It is three-quarters the amount that we put into the National Science Foundation.

I would want to make sure that the Department of Defense took on as a responsibility and continued as a responsibility its support for basic science in this country. It has traditionally been a big support of basic science. It should continue to be a big support of basic science. It depends ultimately for its weapons and its ability to conduct wars on a vigorous basic science establishment. It has a responsibility there. It is discharging it, perhaps not as strongly as many would like to see, but it is discharging it.

Mr. CAMPBELL. With the chairman's permission, one follow-up.

The next potential technological area for a consortium or for a support of this kind is oftentimes mentioned as HDTV. And I know you have expressed some views on that.

I guess my inquiry is, are you of the position that we are in need of government full financing in order to enter that field, or whether the American industrial structure and the technology appropriate for HDTV can make a go of it without direct government support?

Dr. WHITE. I am really not in a position to answer that question. I have not looked at it sufficiently to give you a responsible answer to that.

Mr. CAMPBELL. Thank you very much.

Thank you, Mr. Chairman.

Mr. WALGREN. Thank you, Mr. Campbell.

The Chair recognizes also from California the gentleman from the southern part of the State, Mr. Brown.

Mr. BROWN. Thank you, Mr. Chairman.

Dr. White, I compliment you for the work that you have done at the Academy in exploring a more rational framework for looking at science and technology budgeting. I am inclined to feel that no matter how logical the framework, we are still not going to get better science and technology budgeting until we have a better appreciation of the policy issues and a greater concern with them at the level of the President. I regret that we have not yet seen the appointment of a science advisor who could contribute to that better judgment.

You don't have any secret wisdom as to when we are going to get that science advisory appointment, do you?

Dr. WHITE. We look up to the Hill for that kind of wisdom, sir. [Laughter.]

Mr. BROWN. Right.

Dr. White, in responding to Mr. Campbell, you touched on an area which has bothered me for some time, as you know, and that is how we get over this hurdle of setting policy with regard to the Federal role and the support of advanced new high-technology activities. The question Mr. Campbell raises about should we finance, provide finance for high-definition television is a good example.

I don't know how we are going to solve that problem because it fundamentally goes beyond science and technology to a concept of the role of the Government in the industrial affairs of the Nation, which is not a scientific question at all. It is a question involving political philosophy.

I am wondering if we could get a contribution, however, to answering that question by looking to other countries—advanced industrial countries around the world—just as we might get a clue and have that enter in as one factor in looking at science and technology budgets in general.

Which is obviously Japan, France, Germany, England, you name it. What are these countries doing both today and over time and how does that correlate with their industrial investment, advancement or decline? Are we able to make analyses of this kind and would they contribute to enlightening our judgment on the question of how much we should get involved both in supporting advanced technology and in supporting science?

Does the academy look at that picture?

Dr. WHITE. In the Academy of Engineering we have looked at exactly that picture and have recently issued a statement from our council on the question of technology competitiveness which looks at the whole issue of technology policy that would be usable for our Government.

The view that statement proposes is that technology policy is a broad-ranging, diverse kind of thing which involves the role of the Federal Government in many ways, but places principal burden of remaining competitive technologically in the private sector.

Having said that, it does visualize key roles for the Federal Government. The first several roles are nontechnical. They happen to deal with economic policy of our Government. Unless we can increase investment in plants and facilities which represents the embodiment of advances in technology, then we are not going to be competitive, we are not going to have the economic growth that we want.

And so the first area for Government action is in the whole area of macroeconomic policy, regulatory policy, things of that nature.

Secondly, the second area where we believe it is important for the Government to be very deeply involved is in the question of education. We cannot be competitive over the long run—and this is recognized by everybody—unless we have a vital, vigorous educational system going all the way from beginning, right up to the university level.

Lastly, we see a role for the Federal Government in the support of the civil technology base itself. And that role relates to those areas of technology where, if for one reason or another, market forces cannot put in the resources and investment to enable us to keep up with our trading partners and commercial competitors.

And here we need to have a partnership between the Federal Government and private industry. We don't want the Federal Government calling winners and losers. What we want is a partnership between the Federal Government and the private sector, where the private sector is prepared itself to put up resources—make an investment in technology. And the partnership, with the Government investing resources, also would make good sense to us. In those areas of technology which cannot be advanced through free-market forces alone. So, we see those as general areas, and it represents our view of a role of the Federal Government in advancing our technological capability.

Mr. **Brown**. Well, we have discussed this before. And I think you and I are pretty much in agreement on that. But what it really boils down to, in terms of the ability of private industry to make long-term investments in new technology—perhaps one of the major features is macroeconomic, like interest rates, the ability of the Federal Government to engage in a partnership to say support the basic research that is necessary to this technology.

In fact, it is a device for just lowering interest rates by reducing the amount of capital that the private sector has to put into that particular enterprise. You can raise a real question as to whether we ought to do that and, in effect, we are concealing the impact of those high interest rates and delaying the time that we would take some drastic action—which would mean on the budget and on some other things—to begin to reduce those interest rates.

I, looking at it, would say, from a perspective of a conservative, let's not do anything to support private industry here because the real thing they need is lower interest rates. And we are just postponing the date that we recognize that, if we enter into these partnerships and help them.

Now, that is a rather negative way to look at it.

Dr. **White**. Well, I think as in most things in this world the middle ground is probably where we should be and probably will end up. You know, we have very significant examples in our country of where Government working with private industry has been responsible for promoting and establishing some of our foremost industries.

The aircraft industry is a good example, through the old NACA, National Advisory Committee on Aeronautics, which was a Government investment partnership with industry. So, we have some good examples where such partnerships have worked.

And I fully agree with you, unless we get our macroeconomic policies in such a way—shaped in such a way that we do provide incentives for investment, then no matter what we do in the technological area, it is not going to help. You've got to have both of these things operating at the same time.

Mr. **Brown**. Thank you, Mr. Chairman.

Mr. **Walgren**. Thank you.

The Chair recognizes the gentleman from Pennsylvania, Mr. Ritter.

Mr. **Ritter**. Mr. Chairman, I just arrived, and I would like to wait.

Mr. **Walgren**. I appreciate that.

Let's go to Mr. Boehlert.

Mr. **Boehlert**. I have no questions.

Mr. **Walgren**. I want to go in an informal way. I don't want anybody to feel they are out of turn.

Mr. Skaggs?

Mr. **Skaggs**. No questions at this time.

Mr. **Walgren**. Mr. Slaughter?

Mr. **Slaughter**. No questions.

Mr. **Walgren**. Well, I think perhaps the better part of discretion would be to turn to the next panel at this point.

I do want to express our appreciation to you, Dr. White. As you know, all of us on the panel feel that you are a resource to us, and we can tap you informally and know where to find you. So, the discussions will not stop here, and I appreciate your contributing and starting us off on this record.

Dr. **White**. Thank you, Mr. Chairman.

Mr. **Walgren**. Let's call the next panel.

Dr. Branscomb, who is the director of science and technology and public policy programs at the Kennedy School of Government at Harvard; Dr. Richard Cyert, who is the president of Carnegie-Mellon University; and Dr. Lawson Crowe, professor of philosophy at the University of Colorado.

Gentlemen, welcome to our process. As I said before, written statements will be made part of the record, and you can feel free to emphasize what you would like.

Before starting with Dr. Branscomb, let me recognize the gentleman from Pennsylvania, Mr. Ritter, for a statement.

Mr. **Ritter**. Mr. Chairman, one of the exquisite Chinese water tortures we are faced with in the Congress is the simultaneous hearing process where vital hearings on vital subjects of vital interest to individual members are being held at the same time. I apologize for not being here at the outset.

Mr. Chairman, we are here today to answer a $64 billion question: How could the Federal Government set better priorities to spend $64 billion for R&D? We are building a new economy that is providing jobs and making us more competitive. That is the good news.

Yet, we know we are fighting a global battle for our technological lives and that the Federal component of American science and technology base is crucial.

Two weeks ago Americans focused their attention on Japan during the week of Emperor Hirohito's funeral. Many Americans pondered about our future relative to Japan's and asked tough questions that we need to try to answer:

What makes the Japanese so industrially competitive? What can we do to better use our science and technology base to win global market shares and high-technology, high-value manufacturing products like they have done?

Today, we turn our attention to these questions. I cannot overemphasize the fact that we have an opportunity now to make dramatic progress in how we use science and technology to underpin U.S. global competitiveness. I have seen new attention to the role of science and technology at the highest levels of the Administration. The President's new chief-of-staff, Dr. Sununu, former professor and dean and industry consultant in engineering, and some of his key staffers have scientific and technical training and they are receptive to concrete ideas for priority setting.

Dr. Sununu, let's remember, was also a highly successful Governor of a State whose lifeblood for new jobs and economic growth was high technology and the policies that make it all happen.

Our witnesses from industry, academia, and government will discuss priorities. Let me suggest a modified process. In allocating our R&D resources, we should focus more on long-term national economic priorities that help America as a global economic power just as we focused on defense.

In looking to the long-term, we need to get more input from members of industry and the private sector who have been in the trenches of the global competitive battle slugging it out with other Nations. We need to select leadership personnel in the Federal R&D bureaucracy in such a fashion as to have all key players on the same team with the same goals.

We need teamwork and cooperation between the private sector and government in the educational institutions especially, but not solely, in science, math and language education. If we want to be competitive, we will need effective science education that brings all of America's core scientific concepts and basic understanding.

AAAS has an excellent start in this regard, the American Academy for the Advancement of Sciences. We should be cooperating with them across the length and breadth of the society.

For example, how can we expect our citizens to ask us to spend R&D or regulatory dollars on ways to produce alternative forms of energy when there is so little understanding of the basics. And you could extrapolate to 100 other areas.

We need to be able to support revolutionary science and technology with sustained commitments of manpower and resources. For example, just one example, we must play some role in the world in HDTV or continue to lose out in the ever-expanding consumer electronics marketplace, not to mention potential industrial use of the ever-changing visual medium.

In biotechnology we need to fulfill the promise of genetic engineering or we will remain mired in the laboratory while our competitor nations surge in manufacturing, still the main source of national wealth production.

We need a national reemphasis to make up for three decades of neglect. We must ask ourselves the critical question: Is the organization of Federal science and technology largely within agencies whose missions reflect past priorities, not the current global economic challenge based on technology, sufficient to meet today's challenge?

Finally, even within constrained agency budgets we must attract our share of the best and brightest in science and technology to public service. The reward system in federal agencies is just outdated. It is obsolete. And it is out of proportion, way out of proportion, to the heavy responsibilities that scientific and engineering professionals in the government must assume.

These pressing needs should lead us in a coordinated way to reexamine the length and breadth of the implications of the R&D budget in the Federal R&D economy.

Finally, I would like to thank the distinguished chairman of the Science Research and Technology Committee, my colleague and good friend Doug Walgren, for his years of service to this committee, to this subcommittee, and to this particular issue. I would like to thank him for convening this hearing, appropriately the first of the 101st Congress. I salute you, Mr. Chairman, for recognizing that we do have a new opportunity now to talk about Federal R&D priorities.

I would also like to welcome our panel of distinguished scientists and technical professionals who have distinguished themselves throughout the course of long careers and who speak eloquently on the issues that we are interested in.

Thank you, Mr. Chairman.

Mr. **Walgren**. Thank you, Mr. Ritter, and particularly for those words directed towards the Chair.

The Chair recognizes Dr. Branscomb.

Welcome, Dr. Branscomb.

STATEMENT OF LEWIS BRANSCOMB DIRECTOR SCIENCE, TECHNOLOGY, AND PUBLIC POLICY PROGRAM JOHN F. KENNEDY SCHOOL OF GOVERNMENT HARVARD UNIVERSITY

Dr. **Branscomb**. Thank you, Mr. Chairman.

I congratulate this committee for this inquiry. This committee is a principal locus in the United States for focusing on these strategic issues.

I just returned from a week in Japan, where I was establishing some research collaborations with a new institute, the National Institute for Science, Technology, and Public Policy, called NISTPP, which is an arm of the science and technology agency and supports a policymaking body, the Science and Technology Council of Japan, which is chaired personally by the Prime Minister of Japan. And I can assure you that they are focusing their energies and their best intellects on trying to understand all of these issues. We can do no less for our own people.

I do not need to reiterate the reason we are all here. Gramm-Rudman, Hollings, together with political unwillingness to increase Federal revenues, have reduced the growth of entitlements and makes Federal R&D budgeting a zero-sum game or something very close to it.

The problem is that there are at least seven areas in which we probably should be increasing our investment, and therefore we have to find ways to do this in a more efficient structure of management.

Let me just name the seven ways. I have gone into this in more detail in my prepared testimony.

There are new areas of research of great industrial importance which the academies in their report call "generic applied research", the area of fundamental science and the educational system it supports; a number of long-deferred investments in technical systems and facilities; a set of new big projects to which commitments have already been made.

And there must surely be alternative ways to gain the most value for the least cost, from the space station, the supercollider, the genome mapping, a new fusion power reactor, the aerospace plane, and several others, not to speak of the need for national computer network to support education and research.

Fifth, we need partnerships with industry to modernize key production technologies such as Sematech. The Department of Energy has a new project and a single-process steelmaking technology. And there is the interesting case of high-definition television.

Six, we need Federal-State partnerships, first and foremost in math and science education, but also in Federal partnerships, to help the States encourage the diffusion of existing advanced technology to smaller firms. And the Congress has initiated that mission in the renamed National Bureau of Standards, through the new Technology Administration in Commerce.

Mr. Chairman, your letter asks is the U.S. science and technology effort adequate and balanced. In my view it is neither adequate nor balanced.

How might we evaluate its adequacy? In brief, the absolute size of the annual national R&D effort is about $130 billion. That is three times the national effort of Japan. And by that measure, it might seem more than adequate.

However, 35 to 40 percent of that expenditure is in support of defense objectives. And the nondefense portion is about $80 billion. The size of the U.S. economy, reflected in the GNP, is a little less than twice that of Japan. Thus, nondefense R&D as a percentage of GNP is only two-thirds that of Japan.

Which of these measures, total R&D or nondefense R&D divided by GNP, is a more appropriate basis for comparing the adequacy of U.S. investments with our competitors?

To the extent that research results are difficult to transfer, are proprietary, or are most effectively employed in the institutions that do the work, each R&D effort serves primarily the needs of those that do it. This is essentially always the case in development and is true for most of applied research as well.

It is even more true for military applied research and development and since more than 90 percent of R&D is D, or applied research, the second measure is the more appropriate in the U.S., and the U.S. is underinvesting compared to Japan.

What about balance? I believe there are six dimensions of balance that deserve attention. The first is public versus private. There is too little private R&D in this country by any reasonable measure, as I will point out later. In Japan the private sector invests 1.53 times as much as the U.S. as a percentage of GNP. In Germany it is 20 percent greater.

Secondly, the balance between education and research—and we have problems of balance at the postgraduate level, particularly in engineering education—as it relates to competitiveness.

Third, military versus civil. Here again we have an imbalance that is somewhat inherent in the nature of things. But the cost of keeping these two sides disconnected is very great indeed, and I believe there are opportunities for synergy with different strategies both on the civil Government side and on the military side.

Fourth, the balance between centralized versus disaggregated projects. Some must be centralized; there is no other way to go to the moon. Others don't have to centralized, for which the human genome is the best example. And some perhaps don't need to be embodied in a very expensive and perhaps premature prototype, for which I would cite the X-30 rocket plane and the SDI program.

Finally, there is the infrastructure versus its use. Clearly, we have underinvested in the new tools, the renovation of facilities, and the provision of technical support services. This pennywise, pound-foolish approach is practiced by virtually everybody in Government, from the President and Secretary of Energy, who have awaited urgent recommendations to repair or replace the nuclear materials production reactors, all the way to the individual academic researcher who finds it very difficult to cut salary costs in order to fund the instrumentation to make that work most effective.

Most people don't realize that the capital cost of a research building and its facilities really exceeds one year's research operating costs. We ought to be refurbishing buildings every three or four years if it is substantially affecting the productivity of the science.

The recommendations of the three academies on Federal science and technology budget priorities I think is off to a very good start.

First, I strongly agree that we should try to retain decentralized decisions wherever we can. And that means every R&D program that is aimed at the attainment of unique elements of the agency's mission and could be done within no more than modest budget growth. They should be handled as they are handled today, agency by agency in the appropriate committees of the Congress.

Secondly, I am very strongly in favor of a process led by the President, essentially assisted by an assistant to the President and OMB, for guiding the preparation of the agency R&D budgets, for the very reasons cited by the academy.

Let me turn to criteria now for priorities. And Chairman Walgren's letter suggests three criterias, which I think are quite good, and perhaps they are not complete and we might want to consider the contribution to national—that means private as well as public—scientific and technical knowledge and the capability of its workforce now and in the future.

We should consider contributions to the Nation's security, military as well as economic. We must consider the contribution to people's health, social and environmental well-being, and certainly the international contributions to peace, the global environment, and cooperation with other Nations.

But that list of criteria does not give us an algorithm for deciding which projects to fund or how to manage them, for three reasons:

First, the political judgments about the relative urgency of the social objectives are subjective and certainly will differ and will have to be resolved within the normal play of our democratic process.

Secondly, R&D is not the only way to achieve many of them.

Finally, R&D projects may contribute to them in a wide variety of ways.

I think we must take a more practical approach and ask questions such as does the program have clear technical objectives? If those technical objectives are achieved, will the result have the intended benefits? And are there ancillary disbenefits to consider?

Is the technical plan for achieving the technical objective appropriate, realistic? Does it make the use of the right resources? Are the risks reasonable?

Is the plan for funding and managing the project realistic? Is it designed to maximum learning and minimize cost and risk? If it involves multiple agencies, will the coordination be effective? Should its management be more decentralized, or on the other hand have stronger central management?

Does the project have the right ratio of public investment versus private incentive? Is the availability of all of the resources on which success depends reasonably assured?

Now, these kinds of tests can be applied by experienced R&D managers with good judgment and good technical taste, supplemented by the right experts.

And as a test of the practicality of this approach and the benefits of applying it rigorously, we might even ask our academies to do an experiment and map those kinds of criteria against programs like the space station, the transatmospheric airplane, SDI, and the genome mapping project.

But all of this priority process will founder, in my opinion, if we don't do some serious work on what I call the taxonomy of R&D. And I have been making this point for many years, but I was very pleased to note that the GAO has joined me in that belief and has written several very good reports on this point in which they point out that so long as the categories in which the agencies place their budgets are so poorly related to the motives of the Government in making the investment, then it is very difficult to make these comparisons.

And I submit that the big problem, not just the one that Dr. White addressed, is the fact that on the military side we have a set of criteria, you know, the code, the symbols, 6.1, 6.2, 6.3A, B, et cetera. On the civil side we use only the terms basic research, applied research, and development—terms designed initially by the NSF for studying the sociology of science, not for making resource allocations. And all the civil agencies of Government use those NSF definitions.

The reason this is mischievous is because where the real problem lies, where much of your interest lies, is how the Government determines its role in addressing the research programs that will best help our economy. And I note that the academy has used the phrase "generic applied research." What is that? Is that the same thing as generic technology, strategic research, exploratory development, preproprietary research? These are all terms used in different contexts.

So, I submit we need to look at the taxonomy of R&D, and I do not believe we will solve the problem simply by stripping out the weapons development test and evaluation program. First of all, we have to understand that, when we want to discuss FSX with the Japanese.

And secondly, we have to remember we do those kinds of things on the civil side, not as much as we once did, but take a look at the FAA's air control system, for example.

Let me close by noting that the real harm, I believe, the failure to have such a priority system, making the system exact, is in looking to the future. It is so easy to start big projects with the camel's nose kind of initial appropriation to make a site selection, to get certain constituencies very heavily engaged in the vitalness of maintaining this appropriation, and it will in the future years exact an opportunity cost from a variety of other things we would like to do, not just in science but in the social agenda as well. And I think we really must look forward five or ten years in trying to make these kinds of judgments.

Thank you very much.

[The prepared statement of Dr. Branscomb follows:]

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Committee on Science, Space and Technology,
Subcommittee on Science, Space and Technology
U.S. House of Representatives

Hearing on Adequacy, Direction and Priorities for the American Science and Technology Effort

Feb. 28 1989

Lewis M. Branscomb
Albert Pratt Public Service Professor
J. F. Kennedy School of Government
Harvard University

The committee is addressing an extraordinarily important series of questions in this hearing. Additional or refocused investments are needed in many areas of science and technology at a time when the government budgets - both federal and state - are in serious deficit and the private sector is hard pressed by high capital costs, technical strong and well educated competition. Both our government and our industry must do a better job of managing our scientific and engineering resources.

U.S. Federal R&D Priorities: Post-war history

For three decades after WWII America took its technological leadership for granted. In a spirit of both altruism pride America shared her science and technology with both friendly industrial nations to help their post-war recovery. America shared her science also with poor nations struggling to provide a better life for their people. Much of our talent was devoted to biomedical science - not in the quest for industrial advantage but in the search for cures for disease. We set new standards for creativity in science and engineering with a generous and effective system of competitively awarded, investigator-initiated research grants.

In the process the U.S. created the finest system of postgraduate science and engineering education in the world. Of the one million students - from all nations - who study abroad each year, 350,000 come to the United States.

American science policy has emphasized creativity. Our technology policy featured bold dreams - objectives that seemed out of reach, but in their achievement astonished the world. This policy was not designed to maximize R&D

efficiency, or to give priority to the diffusion of technical knowledge throughout the economy. It was designed to reflect the visions of powerful, self-confident nation.

The main feature of this policy was decentralized decision-making. Market forces were assumed to be sufficient to keep the private sector competitive. Each government agency made budget trade-offs between R&D investments and other means for achieving the agency's mission. When the President and Congress felt a major new project was needed that exceeded normal agency budget growth, additional funds were found with a minimum of stress on funds for other R&D programs. That is, each agency's budget was considered separately, with R&D traded-off against operating programs of other kinds. R&D budgeting was not viewed as a zero-sum game.

Even as recently as March 1988, when Dr. Solomon Buchsbaum and I testified to the Senate Budget Committee on these same issues, he took that traditional position - that cross-agency trade-offs in the R&D budget were not appropriate.

U.S. Federal R&D Priorities: Need for Central Decisions

America now finds itself in a fast changing situation. Gramm -Rudman-Hollings, together with political unwillingness to reduce the growth of entitlements or increase federal revenues, makes federal R&D budgeting a zero sum game, or something very close to it. Any major increase in federal R&D programs does exact an opportunity cost either from other R&D activities or from social expenditures.

Decentralization in R&D priority setting has served America well in the last 40 years. All of us should approach the need and process for making trade-offs among S&T activities with some trepidation. Nevertheless, I believe the need for it is inescapable.

The foreign challenge to American competitiveness calls for expanded efforts and changes in the mix of both private and federal science and technology. Given the budget situation this expansion must be very selective and compensated for by increases in productivity and effectiveness wherever possible.

Five classes of federal support for S&T activities call for added and better focused investment:

1) **New areas of research of great industrial importance need**

more emphasis: Materials research, production process engineering, biotechnology, and information science and engineering are examples of research areas of high economic as well as scientific importance.

2) Serious concerns are raised that we are neglecting the **fundamental science** from which new ideas emerge and through which advanced students of science and engineering are professionally trained.

3) There is a long **deferred investment in technical systems and facilities:** The instrumentation and laboratories in the universities have been neglected. The cost of not keeping up nuclear technology is apparent in the tens of Billions required to modernize the Energy Department's reactors. The family of rockets used both by NASA and the military trace their technology back to the 1960's. Our oceanographic fleet is out of date and inefficient, especially for polar work.

4) Can we meet all these needs and still complete the **big new projects** to which commitments have already been made? Are there alternative ways to gain the most value for the least cost from the **space station, supercollider, genome mapping,** a prototype **fusion power reactor,** the **aerospace plane?** Are there ways some of the technical questions unanswered in the S.D.I. program could be addressed at less cost and with more potential benefit to industry?

Our **national computer networks** that support research and higher education is a hodge-podge, needing a coherent plan and a substantial investment to support switched broad-band service and access to information services that could dramatically improve research productivity.

5) Finally, what will be the experience with federal participation in **partnership with industry to modernize key production technologies,** such as SEMATECH or the Energy Department project in a new steel-making process? If such projects are judged successful and others are identified, such as **high definition television (HDTV),** how will they be funded and managed?

In addition to these five classes of investments, there is a need for **federal-state partnerships** in two areas:

1) **Science, math and engineering education,** where major reforms are urgently needed, and

2) Partnerships to help the **states encourage the diffusion of existing advanced technology** to smaller firms who need to improve the quality of their products and the productivity of their operations. Here the initial investment in the NIST program to provide technology and funds to state-level institutions engaged in industrial extension is only a down payment. If this program, passed in the Omnibus Trade Act, is sound, it will need dramatic expansion.

* * *

Is the U.S. National S&T effort adequate and balanced?

The committee asks whether the U.S. national S&T effort is adequate and balanced. In my view it is neither adequate nor balanced.

How might we evaluate its adequacy?

The absolute size of the annual national R&D effort is about $ 130 billion dollars. This is three times the effort of Japan, and by that measure it might seem more than adequate. However between 35 and 40 percent of that expenditure is in support of defense objectives [1]. Thus the non-defense portion is $ 78 to 85 Billion. The size of the U.S. economy (GNP) is a little less than twice that of Japan. Thus non-defense R&D, as a percentage of GNP, is only 2/3 that of Japan [2].

Which of these measures: **total R&D or non-defense R&D divided by GNP**, is a more appropriate basis for comparing the adequacy of U.S. investments with competitors?

To the extent research results are difficult to transfer and most effectively employed in the institutions that do the work, each R&D effort serves primarily the needs of those who perform it. This is generally the case for development activities and for much of applied research as well. It is even more true for military applied research and development, which is surrounded by security restrictions and is usually conducted in organizations isolated from civilian R&D. Thus the second measure is more appropriate, and the U.S. seems to be underinvesting, compared to Japan.

If one adopts a model in which R&D information diffuses rapidly throughout the economy, then in theory everyone can access the entire body of knowledge and the total national research effort would be a more appropriate figure of merit.

The U.S., with three times Japan's total R&D, would then seem to have an advantage. However, to the extent this is true, the U.S. shares the results with scientists of all nations, hence shares the benefits.

A small part of national R&D is categorized as "basic" [3]. The most fundamental research is, of course, widely shared and takes time to find application, during which time many can learn about it and begin similar work. However, because fundamental research is tightly linked to training, and for this reason is concentrated in the universities, it is essential to perform fundamental research at home.

Thus the ratio of research investments to the production of scientists and engineers should be examined. Looked at this way, it appears the U.S. is not over-investing by comparison with Japan [4].

How about balance?

Several dimensions of balance deserve attention:

1) between fundamental research (mostly federally funded) and education (mostly state and private funded),

2) between military (mostly government funded) and civil (mostly private funded) R&D,

3) between large centralized projects and decentralized and disaggregated research,

4) between investments in infrastructure and tools versus expenditures on their use in research.

Let me consider them in turn.

1) Education and research.

The math and science problems in U.S. K-12 education are not only serious problems, their scale is enormous. Relatively speaking, higher education in science and engineering are in very good shape. To this extent there is an imbalance of major proportions, and the poor quality of K-12 education is discouraging young Americans from pursuing technical careers and is making it harder for the colleges to train them.

However, given the constitutional tradition that public schools are a state and local prerogative, it is not easy for the federal government to redress this imbalance. Federal agencies and the scientists and engineers they support, should, however, join in the national efforts at school reform and do whatever they can to help.

At the post graduate education level is there is one other imbalance. Industry's competitiveness problems are more rooted in design, production process development and quality management than in R&D. The tradition that federal agencies help our post-graduate education by funding research cannot easily be extended to cover the needs for curriculum development in these "downstream" areas of engineering. NSF is already having difficulty supporting engineering science. I suggest that the new Technology Administration in the Department of Commerce should ask industry to join in an examination of what the engineering schools would like to see from the federal government in this area.

2) Military and Civil

Here there is also imbalance, at least in competitive terms. Since the two most effective high-tech competitors to U.S. firms are Japan and Germany, it may be important to observe that neither spends appreciable sums on military R&D [5]. While it is not possible here to go into detail on studies of the relationship between civil and defense technology in the U.S. [6], suffice it to say that except for certain industries where synergy appears to be high and each sector supports the other (jet engines, communications satellites, and to some extent airframes), in most industries the two activities are kept at arms length and the effects of "spin-off" and "spin-on" are weak.

It would appear that secondary suppliers of specialized industrial components, materials and instrumentation to both military prime contractors and large civilian end-product manufacturers do receive significant stimulation from serving both markets. Clearly, to the extent that defense agencies fund basic or generic research that is applicable in all sectors, they contribute to the total national capability.

Thus a reasonable assumption is that with the three exceptions just given, defense R&D has a small effect on civil innovation, and effect that is sometimes positive, sometimes negative.

Thus the U.S. would be much less strained in its total R&D

effort if greater synergy between military and civil industrial capabilities existed. Thus the great interest in dual use technology and the national industrial technology base [6].

3) Centralized vs. Disaggregated Projects

There is no rule of thumb to guide the balance between big national projects, such as Apollo, and the great diversity of smaller projects initiated by individuals, institutions and agencies for an equally diverse set of purposes. Some projects, Apollo being the best example, by their nature require huge organizations and immense, centrally provided budgets.

But there are other examples where one may chose which mode is most appropriate. A current example is the mapping and structural analysis of the **human genome**.

Advocates of a large, centrally financed, national project point to the enormous value to the future productivity of genetic and biochemical research of systematically going after the coding of the genes. The most efficient approach, and the quickest, is to set a clear goal, mobilize a big effort, give it central leadership, and finance it properly.

Critics will argue that this work is painstaking, requires small scale efforts by many small groups who do not have to be in one place or work in a fixed time sequence. They note that three quarters of the genome is thought to be of little importance, and suggest that the best way to allocate research priority is to let the analysis be driven by each researcher's reasons for selecting specific parts to analyze. In this view the best way to get the genome structured at a reasonable rate is to ensure adequate funding for molecular biology as a whole, perhaps with the addition of some focused work on tools for gene analysis and coordination of data collections.

The latter argument also fits better the effort to internationalize the burdens and the benefits from such a mammoth project.

In striking this balance one must also deal with the suspicion of critics that the advocates of a large centralized project may have been motivated by observing they way other fields, such as high energy physics, seem to attract funding for the entire effort by building the program around a few large projects. But if they look at the example of the optical astronomers, who are in serious financial trouble because of insufficient funds not only for the new 8 meter telescopes, but for running the existing national observatories, they may be less enthusiastic about this strategy.

Another issue concerning balance between a few big and many small projects concerns engineering research and practise. Two projects will illustrate the question: can advanced methods and materials be engineered and new system concepts be explored best through broad-based laboratory research, or is it necessary to drive such programs with prototypes of the systems that might result?

To take an example, there is considerable interest in knowing whether a re-usable aerospace capable of reaching earth orbit with only one stage of propulsion is a good idea. It would require a trans-atmospheric ram-jet engine and new structural materials, which would have to be invented. Is the best way to decide if such a development is a good idea to embark on a prototype aircraft development (the **X-30 rocket plane**)? Or would it be more prudent, and perhaps less costly, to combine systems studies with laboratory developments of the critical system components? The same trade off must also be made in the S.D.I. program, although in that case there were political arguments influencing the answer.

4) Infrastructure and its use.

Finally, there is considerable evidence that the U.S. government R&D system is unbalanced in the direction of short term and wishful thinking. Needed capital investments in new tools, and the renovation of facilities, in the provision of new technical support services are too often postponed when the budgets for operating costs are squeezed.

By comparison, private industry takes the opposite view: it understands that people costs, accumulating year after year, are the big expense. Capital investment to make fewer people more productive is almost always seen as a better decision.

Few people stop to realize that the cost of a research building and its equipment rarely exceed one year's operating costs for the research programs conducted in it. Thus buildings should be renovated every few years if it would significantly enhance research productivity. This rarely happens in universities or government laboratories.

This penny-wise, pound-foolish approach is practised by everyone funded by government: from the President and Department of Energy, which has avoided 8 years of urgent recommendations to repair and replace its **nuclear materials production reactors**, to the individual academic researcher who finds it hard to cut salary costs to fund a new piece of instrumentation.

This problem is aggravated because the lack of capital budgeting (with appropriations received for depreciation) in the federal agencies. The lack of central coordination and ability to make decisions binding on multiple agencies also aggravates this problem, as illustrated by the difficulty the FCCST is experiencing with a plan for an integrated, switched, **broad-band computer network** for U.S. research and education.

* * *

Recommendations of the three Academies on Federal Science and Technology Budget Priorities [7]

This document is an excellent beginning for addressing the issues. Several features of this study deserve special emphasis.

Retain decentralized decisions where appropriate.

The document wisely advocates trying to keep as much decentralization in priority setting as is compatible with good management. Specifically, all agency sponsored R&D programs whose goal is closely linked to the attainment of **unique** (emphasis mine) elements of the agency's mission should be judged in relation to other elements of the agency's budget and the total agency budget judged in relation to the priority of the agency's mission. This is the first category in Table 1, [7].

This policy does not require, however, that type 1 projects should avoid critical review by OSTP and OMB.

Establish a process, led by the President, for guiding preparation of agency R&D budgets.

Second, the document proposes a process that in no way diminishes the responsibility of the OMB, but greatly increases the likelihood that the President will see a more balanced and coherent science and technology program in his annual budget. Furthermore, he will be able to receive from his Assistant for Science and Technology an analysis of how the total budget affects the health and vitality of the nation's S&T enterprise as a whole in time to alter the budget if necessary.

Categories for Analysis and Priority Setting

I have already supported the first category, Table 1, as a means for retaining the maximum decentralization in decision making.

The second category: the S&T base, requires a mode of analysis across agencies that will permit the effect of the total federal program on the institutions of S&T to be evaluated. In this category are all the things that are vital in the aggregate, but few agencies can alone sustain the needed national capability.

This category should not be restricted to "basic research" or "6.1 and 6.2" in the DoD budget. For many programs, including defense procurement and setting of technology regulatory standards have direct effects on the S&T system of the nation. Thus I see category 2 not as a list of projects, but a template each agency should put across its proposed actions that would have major effect on the S&T system.

The opportunity for the "tragedy of the commons" operates here. If the Department of Defense proposes in its budget to cut funding in the 6.1 (basic) category of research, a mechanism is needed to test the consequences for other national interests and perhaps rectify the deficit with investment elsewhere.

If this element of the process were put in place, the President could ask his Assistant for S&T to charge the National Science Board (perhaps with help from NIH), as well as the Academies, how best the OMB and OSTP could set guidelines and make this analysis.

The third and fourth categories are essential of a piece, with category four referring to new programs not yet committed. The category three projects cover essentially all of the R&D budget except education, fundamental research and research facilities. (The special case of unusually costly basic research facilities such as the SSC clearly must fall in category three or four, as would category one projects of unusually high cost, such as the DOE reactors.)

The report does not estimate what fractions of the federal R&D budget are likely to fall into each category. Perhaps this is the follow-on exercise that should be attempted. It is my feeling that as a practical matter, one should begin this process with as many programs as possible in categories 1, 2 and 4, having selected a small number of specific, cross-agency programs plus others of unusually high cost implications for category 3. Progress on a few such issues would greatly increase the confidence both Congress and the agencies will place in this system.

Criteria for priorities

The committee requests advice on the main external criteria for evaluating competing projects. Chairman Walgren's letter suggests three criteria: (a) relevance to civil technology, (b) broad societal benefits, and (c) advancement of other areas of science. These are appropriate but incomplete. I would prefer the following:

(a) contribution to national (ie private as well as public) scientific and technological knowledge and capability, now and in the future;

(b) contribution to the nation's security, both economic and military (which are closely interrelated);

(c) contribution to the people's health, social and environmental well-being;

(d) contribution to the nation's obligation to preserve peace, protect the global environment and cooperate constructively with other nations.

A list of such criteria will not, however, provide an algorithm for deciding what projects to fund, for three reasons:

* political judgements about the relative urgency of these objectives may differ,

* R&D is not the only way to achieve them, and

* R&D projects may contribute to them in a wide variety of

ways.

Taxonomy of R&D

For this system to work, there must be more useful and coherent definitions of different types of S&T activities that show up in federal budgets. Two thirds of the programs (defense agencies) use one system for constructing budgets, and one third (all the civil agencies) use another. The General Accounting Office is called attention to this obstacle to setting priorities in the federal budget, but nothing has been done in response to their reports [8].

The problem is illustrated in the report from the Academies. They refer (p. 8) to "fundamental" and to "generic applied research", neither of which corresponds to any federal R&D budget category, although "fundamental" probably means "basic" and "6.1". But what is "generic applied"? In other contexts one encounters "generic technology", "strategic research", "exploratory development", and "pre-proprietary research".

These words and phrases grope for language that all can use to talk about the kind of federally sponsored research that is likely to be of great interest to industry while not raising fears of inappropriate government interference with the competitive market-place. It is precisely in this area that the priority arguments are most likely to be made. For what do the SSC, the Space Station, the Genome project and the X-30 have in common? The hope that each will strengthen American industrial muscle and make us not only a prouder but a more competitive nation.

Finally let me note that in their appendix, the Academies make the same plea: but they restrict themselves to suggesting that perhaps because Test and Evaluation are technical activities of the military with few civilian government counterparts (how about FAA? new drugs?) their inclusion in defense RDT&E exaggerates the technical effort of a civil sort that is meaningful to industry.

Their suggestion has merit, although it is easily addressed by sticking to categories 6.1, 6.2, 6.3, and 6.4 when comparing expenditures with other parts of the government or with other nations. It is more important, in my view, to review the system used by NSF and adopted by other civil agencies: basic research, applied research and development.

Com on Sci. Space & Techn.
L.M.Branscomb Feb. 28 1989

Looking to the future:

Many of the priority decisions for the current year are narrowly constrained by past decisions and current political or budgetary realities. The most important purpose of such a priority setting exercise is to anticipate which programs will need to grow in future years, which might shrink.

Thus when the space station or the X-30 are being thought about, it is their costs 4 or 5 years from now that are important. At that time, will the industrial competitiveness problem be so severe that government will want to increase dramatically investments in partnerships with industry, such as SEMATECH or the proposed HDTV project? Will there need to be major growth in generic research of industrial importance, such as materials science and engineering? Will new strategies for improving the contribution of civil industrial technology to defense technology offer a chance to reduce defense R&D?

The major failing of the current system for handling the federal R&D budget is that decisions to initiate highly visible projects with strong political commitments preempt alternative future investments.

Footnotes:

[1] Obligations for defense R,D,T&E in 1988 were $39 billion, to which must be added private sector IR&D investments and an estimate of R&D expenditures of firms that sell products to the Department of Defense but do not take R&D contracts or receive IR&D allowances.

[2] In 1985 U.S. non-defense R&D expenditure was 1.9 % of GNP, while in Japan the corresponding figure was 2.8 %. National Science Board: **Science and Engineering Indicators, 1987.** Washington DC: U.S. Government Printing Office, 1988

[3] In both the U.S. and Japan about 12 to 13 % of national R&D is categorized as "basic".

[4] The U.S. has about the same fraction of scientists and

engineers in the labor force as Japan, which has about half the population. Thus, if we take the figures at face value, the U.S. basic research expenditures (public and private) per employed scientist or engineer is about 1.5 times that of Japan. Comparing numbers of graduate students, which is higher in the U.S. by substantial margins, these ratios are about in balance. See table 3.5 in Science and Engineering Indicators, 1987, ibid.

[5] In Japan, military R&D expense is 0.1% of national R&D according to the National Institute for Science and Technology Policy (private communication, Feb. 21 1989). In the U.S. the number is more like 40%.

[6] A project at Harvard's J.F. Kennedy School of Government is devoted to understanding the dynamics of the generation and diffusion of technology in military and civil sectors and their relationships. I would be pleased to provide the Committee with a briefing on this work at its request.

[7] **Federal Science and Technology Budget Priorities, New Perspectives and Procedures,** National Academy of Sciences, National Academy of Engineering, Institute of Medicine, Washington DC: National Academy Press 1989.

[8] L.M. Branscomb, **Policy for Science and Engineering in 1989: A Public Agenda for Economic Renewal,** American Academy of Arts and Sciences Bulletin, November 1988.

Mr. **Walgren**. Thank you, Dr. Branscomb.

Dr. Cyert?

STATEMENT OF DR. RICHARD M. CYERT PRESIDENT CARNEGIE-MELLON UNIVERSITY

Dr. **Cyert**. It is always a pleasure to talk with this committee on a problem that is clearly an extremely important one, an interesting one intellectually, and an extremely important one for our society.

Looking at the problem as a whole, the real difficulty in making allocations on a rational basis is that there is no set of overall objectives to which the research is speaking.

We last had an attempt to develop a national set of objectives, I believe, during the Eisenhower Administration. So that I think one of the things that is really lacking from your standpoint in the ability to make these decisions on a better basis is an accepted set of national objectives. I would urge the possibility of a national commission on objectives that would attempt to speak for the country and work in a way that would enable a lot of voices to be heard, to develop a set of national objectives.

Out of that set of national objectives then one could begin to make some determination as to which allocations of research would help us achieve those national objectives more specifically.

I would add to the set of national objectives an exercise—that would attempt—that would really do technological forecasting to determine which areas in science have potential paybacks within some given period of time, to try to determine the degree of importance of these, the length of time, limitations in the realization of these objectives, and a series of other criteria, including how these sciences would relate to our national objectives which would be developed.

And I would urge that this forecasting be done in a way that would attempt to utilize a fair number of people in the whole process. There is a technique called the Delphi technique, which you may be familiar with, which is essentially a way of bringing about through making forecasts, giving feedback, and doing the forecasts again to bring about some sort of conformity and focus on the forecast.

And I would submit that having this sort of forecast so that you had some sort of feeling for what was going to be happening in the future in particular fields of science, in the view of this scientific group—and I am thinking here of doing this kind of questionnaire study, forecasting study, say with maybe 5000 people in the United States, maybe using some samples from the National Academy of Science, the National Academy of Engineering, Institute of Medicine, and some group of people outside the academies—to try to get a good technological forecast that would give you an idea of what are the important fields and where is there going to be, in the view of the people in the field, potential breakthroughs.

Being armed with this set of objectives and with some understanding of what the future looks like in a variety of fields and where there may be breakthroughs and what the time constraints are in these breakthroughs and what the limitations are, you would have information which would enable you, I think, to do a better job of allocating on a rational basis the dollars to specific areas.

So that I am strongly in favor of your getting some overall set of objectives and some understanding of the way in which science is going, in order to have a rational basis for resource allocation.

Now, I think there are a number of other very interesting questions that have been raised. And I was particularly interested in the questions of individual investigators versus the large group projects. I think that it is vital in this country to keep the certain amount of funds to be available for the individual investigator. This is one of the ways in which we can tap the best thinking of people in a variety of scientific fields. It is a decentralized method of really determining what are important research objectives. I think that, combined with the kind of peer review we have in NIH and in NSF, can give us some guarantee—perhaps guarantee is too strong a word—give us some hope that we can have a program that is not going to miss a lot of areas because of some centralized decisions that might be made on what is important and what is not important.

I think the questions of the adequacy of research are best answered, in my view, not necessarily by comparisons with others—although obviously Japan is a very obvious place to make some comparisons—I think that the real test or the best test, in my view, is the number of good ideas in the country that are going unfinanced.

I think the recent competition for science and technology requests for proposals is very good proof of, or evidence at least of, the fact that we are not coming close to financing many, many important and significant ideas that have a real chance of making contributions to science and to our economic development. So that I would certainly agree that on this basis that we are underfinanced in science, and I think the NSF competition on centers of excellence in engineering is another example of the fact that we have more good ideas than we have financing of these ideas. So that I am definitely in agreement with Dr. Branscomb that our scientific endeavors are certainly significantly underfinanced.

Now, I think that the success that the Japanese have had in economic development is certainly something that creates anxiety in this country. We make invidious comparisons with the Japanese, and this is probably a good thing for us. We need to be shaken up and need to be relooking at a number of activities. But I think it would be a mistake to believe that what we should do is imitate the Japanese.

One of the characteristics of our society at this point is it really is a knowledge-based society. Economic development is dependent on knowledge and not on natural resources any longer. It is really a function of the brainpower of the various peoples in the country.

But knowledge does spread rapidly, and it is going to be very hard as we move to the future for any one country to maintain dominance in the economic sphere for a long period of time. The knowledge does spread, and there will be imitators and the position certainly of any one country will be weakened. It is a process that was described by Joseph Schumpeter on innovations within a country. Imitators come along of[1] any sort of monopoly position that an individual may have had or, in this case a country.

But I think it is true that for the long run those countries who have a pool of knowledge to which there can be access are going to be the countries that are able over the longer run to maintain strong positions. So that we need to continue to develop this pool of knowledge through a support of basic research, and we need at the same time to look at our institutions that relate to technology transfers. And we have to strengthen those institutions. And I think the NSF under Erich Bloch has been doing an excellent job of moving in that direction.

So that the development of this pool of knowledge is going to be of great importance to us, and I think that in that development we don't try to do that the way in which, say, Japan does with MITI. But rather we continue to try to use our system which does depend on decentralized decisionmaking.

I have argued for some time that we should have our own version of MITI, and that version, I believe, can be dependent on universities and corporations and Government cooperating.

Specifically, I am thinking of the following sort of process: There are a number of universities in different areas that have maintained strengths where they have a comparative advantage. For instance, Carnegie-Mellon was one of the few universities that maintained strength in the whole magnetics area, and so we were able to put together a consortium of firms to do research with those firms in the magnetics area—and the magnetics technology area.

These firms—it is really organized like a little MITI—these firms make a contribution. The university makes a contribution. The research goes on. The firms have some voice in the research schedule and the research program, but the university retains veto rights on that. The patent rights are taken out by the university, but the participants have royalty free licenses.

The thing that is missing is the leverage that would come from Government funds to this consortium. And the thing that I have been arguing about or pushing for is that we should have our own version of MITI which would be a decentralized version where firms and universities would get together where there was a particular comparative advantage that a university had, and then these proposals would be evaluated by a peer group and those that passed would then get funding which would enable the kind of leverage to be made on a particular project that MITI is able to make with Government funds.

So, it would be our own version of MITI, and I can see a number of decentralized projects of this kind that could over time make a very major contribution. The question of high-resolution television, for example, might be one of these kinds of projects. And we would then be able to operate, I think, much more closely to our own kind of system and to utilize the strengths that we have for decentralized science in an effective way.

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[1] After reviewing the transcript, Dr. Cyert indicated that "of" should be replaced with "to weaken" to express his intended meaning.

I think this is really—I have attached an appendix to my testimony which describes that particular idea.

And I think that is really all I have to say at this point.

[The prepared statement of Dr. Cyert follows:]

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TESTIMONY BEFORE THE COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY

U.S. HOUSE OF REPRESENTATIVES

Richard M. Cyert
President
Carnegie Mellon University

February 28, 1989

The problem of determining the portion of the gross national product that should be spent on research in science and engineering is difficult to solve precisely, as is the determination of the proportion of the federal budget that should be allocated to research in science and engineering. As one looks at the world scene, however, there are certain patterns that are clear and that give us advice on the direction of the trend that should be underway in the national science and technology effort.

It is clear that knowledge is becoming increasingly important in economic development. No longer is the economic status of a country determined by its natural resources, as may once have been true. Now, it is knowledge, regardless of natural resources that are available. Japan stands as the example of this proposition, since it is a nation with few natural resources besides the brains of its people. Through knowledge, we are able to develop materials that make us increasingly independent of nature. This knowledge has another characteristic, which is that it spreads rapidly across national boundaries and quickly becomes the property of the world. It is difficult to gain a monopoly position through knowledge. It is always possible to modify what is done so that patents can be voided and competition provided. Thus, I see future national trade as being increasingly competitive. One country will have a temporary advantage because of discoveries within that country or technology that has been utilized even though it may have been developed in another country. Whatever is done, however, will soon become widely known, and other countries will come to imitate and compete. This is a famous pattern once described by Schumpeter with respect to innovation within a country. The same pattern will prevail on a country basis. This means that one country will not maintain a dominant position in international trade indefinitely.

However, the country that has a large pool of knowledge into which its companies can reach and extract technology that can be commercialized will be the country that will return again and again to a leadership position. This pool of knowledge is clearly developed by the country's research capabilities. Thus, it is increasingly important that higher percentages of the gross national product find their way into research processes and that we also recognize the need for spending time on the institutional aspects of technology transfer. With a strong research program in the basic areas, as well as the applied areas, and with an emphasis on technology transfer, any country will be able to maintain a leadership position for some portion of the time. More generally, such a country will be able to maintain itself as a competitive factor in international trade and will be able to develop an increasingly improved standard of living for the people of the country. It is difficult to assess the adequacy of the effort that the United States is making in research in science and engineering. However, it is clear that the basic research of the country is being done increasingly within universities and that less and less basic research is being done within industry. This result is a reaction of industry to the kinds of financial pressures for short-run results that are being exerted by financial markets. This increased pressure on universities to perform the basic research of the country has developed over a period of ten years or so and has resulted in enormous pressure being put on universities for more and better facilities. At the same time, many industrial research facilities are becoming available and are being sold for other uses. Usually, these facilities are too far away from universities for them to be utilized by the universities, but a program designed to give firms incentives to attempt to get unwanted research facilities transferred to universities may be something worth examining. The amount of funding available in a number of areas, particularly the materials area, is probably inadequate. The inadequacy would be judged by the number of excellent researchers who would be turned down for grants and who would not be able to pursue ideas of potential interest.

It is true that we have more good ideas in this country in relationship to science and technology than we are able to finance. The recent competition for science and technology centers, run by the National Science Foundation, is an example of the inadequacy of the funds being allocated to research. There were a large number of centers that were viewed to be worthy of support that had to be turned down because the funds were not available to support more than a limited number. The same would be true of the centers of excellence in engineering competition that the National Science Foundation has run. Ideas relating to the improvement of teaching in the physical sciences are also badly underfinanced. We are not getting the innovations in the teaching of the sciences because of the lack of resources for changing designs and making improvements in laboratories. Industry, through foundations and other approaches, is not picking up the slack in the government financing in these areas.

It is also my view that we are probably underfinanced in basic research in the sciences. Again, we have a larger number of good ideas and good researchers who cannot be financed because the funds are not there. These statements are true of life sciences, physical sciences, information sciences, and social sciences. Obviously, no nation can put enough resources into research to finance all the good ideas. It would be, however, enlightening for this committee to get reports from the National Science Foundation and the National Institutes of Health on where the cut-off point is with respect to research to find out how many good proposals the people from these agencies feel are being neglected because of the lack of funds. From committees associated with the NSF and the NIH that I am on or have been on and from my knowledge of the proposals submitted by Carnegie Mellon, I have to believe that we need more funding in the NSF and the NIH. The NSF, under Erich Bloch, has done an excellent job of putting more money into technology and, given the current rate of funding, the balance is close the proper one. However, additional funds should be made available to the NSF to help increase the amount of research in some of the scientific areas.

In this regard, it would be informative for this committee to have a study made in the way in which the Department of Defense allocated its research funding going back to the 1950's. I am particularly impressed by the way in which the DoD has dealt with the problem of computers. Without the kind of funding that the DoD made in computers, this country would not have its leadership position in the computer field. The DoD concentrated the funds that it put into computers in a relatively small number of places. It put in large sums of money into these places, and it did not require the investigators to perform classified research. It was able to utilize good academics and to allow them to publish freely so that there was a good exchange of information among interested people. In fact, the success of the computer research financed by the DoD was the result of their being able to put in millions whereas those schools dependent on the NSF could operate only in the hundreds of thousands. The computer field is an expensive one, and the DoD, by its policy of non-classified research and adequate money, has given this country a magnificent comparative advantage in computers. This pattern which I have described only in a macro way should be studied, and a great deal could be learned about ways in which improvements in research by allocations could be made.

You asked for specific comments on the National Academy of Sciences' report. The major points that the individuals from the Academy have made are excellent. It is certainly true that the work that each agency can finance should be known in a broader way to other agencies. Without an increase in the amount of information across agencies, the research activities of the country cannot be properly coordinated and financed. We need to look at the total amount of money available for research, regardless of the agency, and at the way in which those agencies tend to allocate their funds. In this fashion, by having a framework for examining the total research in science and technology regardless of agency, the science advisor or some small group at the top can have a better notion of the weaknesses and the gaps in our financing. This point is the most significant objective of the recommendations that the National Academy of Science is making.

The most difficult problem in the allocation of funds is the development of criteria for choosing and evaluating the priorities of projects across scientific fields. The allocation of public funds in science and technology should relate to the objectives of the nation. Unfortunately, we have neither a clear statement of national objectives nor a ranking of the importance of the objectives. Is an increase in economic competitiveness, for example, more or less important than an increase in our national defense capabilities? If those two objectives are not difficult enough to choose between, think of adding the question of eliminating cancer or, in general, improving our knowledge of medical science.

A brief look at some data concerning the allocation of funding may help to set in our minds the importance of developing proper criteria for priorities of projects. For example, in fiscal year 1980, the total federal research and development budget was $29.8 billion. The split between defense and civilian funding was about 50/50. Yet, in fiscal year 1988, federal research and development spending grew to $58.8 billion. However, 84 percent of the increase was for defense research and development. It is clear that civilian research and development from 1980-88 has not kept pace with inflation. In fact, it has been cut more than 9 percent in real terms during that time.

Civilian basic research has grown from $4.7 billion in fiscal year 1980 to $9.5 billion in fiscal year 1988 or a 102 percent increase. Health research accounts for nearly one-half of the total growth in basic research through 1988. On the other hand, defense basic research experienced 13 percent real growth through 1988, signifying an emphasis on applied research related to procurement.

We need a process by which national objectives can be stated and, in some sense, ranked. The last time this was attempted was during the Eisenhower administration, and I am not sure it was particularly successful at that point. However, again, it would be useful, from the standpoint of this committee, to urge that such a national commission on objectives be established. Failing such a statement of objectives, one can only express personal views. At the moment, the most important national objective is increasing our economic competitiveness.

Therefore, I would like to see the allocation to science and technology use the criterion of "increasing national competitiveness." I use this term, even though it is itself a vague term and needs to be better defined before one can determine the relationship between a particular allocation and achieving the objective. More specifically, I am thinking of ways of improving our capacity to manufacture on a competitive basis and of improving our productivity when speaking of "increasing national competitiveness." I would use this criterion to choose among projects from different fields of science and technology. However, using a criterion of this kind is going to lead to highly applied projects, and the total science and technology research budget should be allocated to projects that are highly applied. Some of this work will get done in industry. Thus, even using this criterion, one sees a need for a balance between basic and applied research that will relate to national competitiveness. At the same time, we do not want to allocate all of our funds on the basis of one objective. Thus, we need to get a ranking of our objectives and use that ranking to guide ourselves in the allocation of research funds, bearing in mind that we also want to be building for the future, as well as trying to solve current problems.

In order to have a better understanding of where to place funds with respect to current and future prospects, we must have a sharper understanding on where science and technology are going in the future. In other words, what are the trends in technological innovation?

Another set of criteria for allocating resources among various projects can be developed by gaining knowledge on where major breakthroughs may be expected in the future. In other words, we should use a criterion for applying money now to projects that have the greatest possibility of desirable major breakthroughs in the future. To identify those potential breakthroughs, the best approach is to use technological forecasting. There are a number of people who have made a speciality of technological forecasting, and it might be useful for this committee to have a panel of such people and ask them to make forecasts.

Another approach to the same problem is one that the Japanese have used through a technique called Delphi. A sample of people from the National Academy of Engineering, the National Academy of

Sciences, and the Institute of Medicine might be selected. These people might then be asked a series of questions involving their views of where breakthroughs are expected and also a number of other questions relating to the role that additional funds might play in hastening these breakthroughs. It is a technique that we could use effectively in this country. Such an approach would give this committee a great deal more information on which to make allocations among agencies and also to judge the effectiveness with which agencies are allocating funds.

It is, in all the allocations that are made, important to continue to have funds available for the individual investigator or for pairs of investigators. Much of science has become "big science," and many groups of people are involved with large amounts of capital. However, many discoveries, perhaps the bulk of discoveries, have come from "small science." Individual investigators, working in the lab or at their desks, have been able to develop new knowledge in an important way. After the basic knowledge has been developed, projects frequently become large in an effort to exploit the breakthrough. To have funds available for the individual investigator and to have these evaluated by peers is the ideal way for science to develop. Individuals who make proposals are stimulated by their own curiosity and desires and will do good research. This approach is also one that gives the United States some protection against areas being overlooked and good ideas not being exploited. As much as possible, we want to have the actual research projects developed in a decentralized manner by investigators who are interested in doing the work. That is the way to get science done.

We also want to avoid allocations to research that follow Japan's method, MITI. We do not want to target areas that are centrally developed by a group of people and then mobilize resources for these projects. However, we can get the equivalent of the MITI approach through effectively using cooperative research between universities and corporations. I have attached as Appendix A a description of a process that is one that should be adopted by the country as a further method of the allocation of research funds.

Furthermore, I want to stress the importance of establishing the science advisor's office in the Oval Office inner circle so that the advisor can have clear responsibility and authority for setting priorities in science and technology in this country.

It is essential to the economic welfare of our nation that we recognize the importance of these two functions and act quickly to establish them.

Universities Functioning as MITI

Richard M. Cyert

Japan is generally viewed by most of the population of the United States as the greatest economic threat existing in the world. We have seen Japan develop from a country that was noted for its cheap and poorly-constructed products to one that is noted for its quality products. Further, Japan seems to be moving into almost every area of manufacturing. It appears that no industry in the United States is safe from Japanese competition.

Much of the activity with respect to specific industries seems to develop through the organization known as MITI in Japan (The Ministry of International Trade and Industry). It functions, in part at least, as a kind of national planning agency for the country. When an industry in the United States is hit by Japanese competition, it usually turns out that the industry has been targeted by MITI as one in which the Japanese can make an impact. To the extent that Japan has a so-called "industrial policy," MITI seems to be the source of it.

The concept of an industrial policy has been generally rejected in the United States. The notion that some agency of the government would select industries that are to be favored in some way and supported to insure their success is alien to the market philosophy which is the basis of our economy. As a result, many firms see themselves fighting foreign governments in their attempt to survive and prosper while their own government gives them no help in the struggle.

This position is particularly evident with respect to research. MITI, once it has decided on a target industry in which it believes Japanese firms can compete successfully in the United States, forms a consortium of Japanese firms. A research agenda is developed. Each of the firms makes a contribution to the research project; and the Japanese government, through MITI, is able to leverage these contributions with government funds. When the project is completed, each of the firms is free to take the results on a royalty-free basis, and they compete with each other--but, more specifically, with American firms in the American economy.

This procedure is a reflection of the importance that the Japanese place on research. The government officials realize that research is the lifeblood of the economy and that some research will not be done unless all of the firms are cooperating. This type of situation is particularly relevant the more basic the research is. It may be difficult for any one firm to utilize the results of the research and, therefore, it tends not to be done. Whereas, if all firms will cooperate and all get the results, then the research may well be completed. MITI has been extremely successful in the consortia that it has established.

In the U.S., as I have indicated, we trust to the market system and do not have anything resembling an industrial policy of the type that MITI has established in Japan. At the same time, it is clear that in a number of industries research is needed and is not being done because no firm can capture the results of the basic research. Further, as the Japanese have made inroads into our industry, firms have reduced the amount of research they are willing to perform. Research tends to be the first area that is reduced in a cost-cutting approach by firms. Thus, many of our industries become even more vulnerable to the type of policy that the Japanese are following.

The answer to the problem is not to mimic the Japanese. There are significant cultural differences which would make it difficult for us to be successful if we were to copy the Japanese. The key in any strategy is to develop approaches in which you have a comparative advantage. A major comparative advantage in the United States is the ability of universities and corporations to work together. In particular, there has increasingly been an effective research cooperation developing. No other country in the world has the same kind of relationship between its corporations and its universities. Thus, our approach should be to utilize the concept of MITI in a context which is American and in which we have a comparative advantage.

Specifically, I propose that research universities, where appropriate, function as MITI does. A university with a particularly strong research area should form consortia with the firms in the industry. Each firm should agree to contribute research funds to the consortium for a given period of time. A research advisory committee should be formed from the industrial members to have an input on the research program. The university must have the final power to determine the research program. Hopefully, with an advisory committee, firms would be able to make adequate inputs.

When the consortium is formed, a proposal should be made to the National Science Foundation for additional funding to the university. This proposal should undergo peer review as other research does. When the consortium proposal is approved, the university would then have additional funds that could be used to leverage the industrial contribution, just as MITI has additional government funds. The net result would be that we would have a number of MITIs around the country, and we would be getting the kind of research done that we need to save a number of our industries. The firms would be contributing but so would society through the National Science Foundation funding. The consortia would have passed good scientific review, and hopefully, a number of them would be successful. The university would be in a position to take out the patents, and the firms would get royalty-free licenses on the work. What is needed is an acceptance of this procedure and a commitment of funds through the National Science Foundation to carry out the proposal.

One needs to do very little analysis to see that the future of American industry is closely linked to research and the resulting knowledge. We cannot compete with most of our international rivals in processes that require a significant amount of labor. The cost of our labor is just too high. Our hope lies in superior knowledge for producing products, for developing new products and improve quality, and for advanced management techniques. Universities have by default become the major places for research in all of these areas. We must have the will and courage to invest in research and have faith that the resulting knowledge can, indeed, make us again a powerful industrial country.

2.11.86

Mr. **Walgren**. Thank you very much, Dr. Cyert.

Dr. Crowe?

STATEMENT OF DR. LAWSON CROWE PROFESSOR DEPARTMENT OF PHILOSOPHY UNIVERSITY OF COLORADO

Dr. **Crowe**. In a recent issue of American Science, John Dutton and I published an article presenting a procedure for setting priorities among large-scale scientific initiatives. We believe the scientific community should accept responsibility for proposing a coherent, balanced, and fiscally realistic national science agenda that would set priorities among proposed initiatives.

The national science agenda would enable Congress and the Federal agencies supporting research and the Administration to distinguish between those proposals that appear to be mandatory and those which offer grand, new intellectual vistas or significant technological breakthroughs and those providing for ongoing activities essential to support and strengthen American science.

Much has been said about the reluctance of scientists to evaluate activities outside their own disciplines. It has also been noted that they are sometimes reluctant to assess the possible social benefits of their work.

But Dean Dutton and I argued that criteria can be established which make evaluation of large-scale scientific initiatives by scientists feasible and effective.

We offer a rationale, a set of evaluative criteria, and a procedure for the application of the evaluative criteria. This procedure and these criteria are designed specifically to evaluate and place in priority order large-scale multidisciplinary initiatives requiring substantial funds sustained over a significant period of time.

We specify three broad categories of criteria necessary for the evaluation of scientific initiatives. They are: scientific merit, the possible benefits to society of the initiative, and the programmatic implications of the initiative.

With respect to scientific merit, I will not say very much here except that it is obviously a necessary condition for an initiative to be undertaken.

In respect to the social benefits that might be derived, it seems the case that almost every large-scale initiative under consideration at the present time has the potential for social benefits. These benefits range from improved international relations to various new technological applications. They also include other valuable human activities: economic development, improvement in health care, improvement in the national defense.

In regard to the programmatic implications, these entail understanding of the readiness to execute an initiative. This refers to the availability of competent scientists, technicians, support staff, and facilities.

We also consider overall costs, opportunity costs, the impact of the proposed initiative on industry, on higher education, on the Federal scientific laboratories, and on international collaboration.

We considered many of the criteria mentioned by Dr. Branscomb in his testimony this morning. We believe that the formal use of explicit evaluation criteria in these categories at the highest scientific decisionmaking level is superior to other present informal competition and procedures currently followed.

This is because a formal evaluation procedure will require the supporters and proposers of an initiative to produce answers to a standard set of questions. They will have to address the same issues. Their rhetorical skills will not be as significant under those circumstances, and meaningful comparisons among initiatives will be possible.

Formal evaluation depends on the informed judgment of the evaluators. It diminishes the effects of hidden agendas and covert self-interest.

It is true that there are a number of numerical evaluation schemes in which proposals can be scored and rank ordered with respect to each broad category of criteria, and they are relatively easy to develop and apply. Some scientists and perhaps some Congressman object to numerical evaluation schemes because they claim that such schemes allow external criteria to be considered.

This objection amounts to saying that scientific merit alone should be the basis for achieving an initiative or advancing an initiative. And we do not agree. Scientific merit is the necessary condition for accepting an initiative, but it is not the only one. When two or more proposals are equally meritorious in scientific terms, and they frequently are, but one promises greater social benefit or one is programmatically superior, these aspects need to be made explicit in the evaluation process.

We believe that initiatives scoring highest in all three of the broad categories of evaluative criteria are most likely to be attractive at the national decision level.

Thus, we propose a formal evaluation procedure for scientific initiatives, using a specific set of criteria based on these three broad categories. The criteria will be phrased as questions to be answered in formal written proposals to be compared as part of the priority ordering process.

This procedure is designed to permit a scientific advisory body too compare a set of large-scale initiatives and to reach an unambiguous conclusion or an unambiguous set of priority recommendations. It is designed to be of greatest usefulness for nonscientist decisionmakers in Federal agencies and to provide the quality of scientific advice now unavailable to Congress and the Administration.

Let me summarize quickly the issues covered by the criteria. In respect to scientific merit, our questions deal with the scientific objectives, the significance of the objective, the breadth of its interest, its potential for new discoveries, its uniqueness and timeliness.

In respect to social benefits, the questions deal with contributions to the improvement of the human condition, to economic growth, to national security, to technological applications, to the public understanding of science and the international understanding of science, and to national pride and prestige.

In regard to the programmatic concerns our questions concern technological feasibility, the availability of facilities and personnel, direct and indirect costs, opportunity costs, and possible international involvement.

The application of these criteria through the procedures we described is fairly straightforward. It is familiar, in fact, to many who are accustomed to ranking scientific grant proposals. An appropriate body of evaluators needs to be appointed, and it needs to be given a specific charge.

The advisory body then needs to determine the scope of its work and the schedule of its activities and the evaluation system to be employed. There is obviously more than one way to set up an evaluation system, and we recommend the criteria that we have described.

The advisory body needs to discuss the main issues in the written proposals and then each member individually ranks the proposals in the light of the broad evaluative criteria.

If a consensus or strong majority opinion is reached, the preparation of priority and budget recommendations can proceed.

My colleague, Dr. Dutton, has applied this procedure successfully with two high-level advisory committees, one at NASA and the other at the NSF UCAR long-range planning committee. This latter application was especially notable since it involved consideration of multidisciplinary long-term initiatives in the atmospheric sciences. The evaluators were university scientists, UCAR trustees, representatives from NOAA, NASA, and DoD. The findings of this committee were reviewed by other appropriate bodies, and they have been widely accepted. I have included a copy of this report for the record.

Therefore, in light of the many scientific opportunities that we have before us, some which appear to be mandatory, some on a grand scale, some incremental, and in light of the need to continue support for basic research in graduate education, it appears that the Nation needs to establish a long-term plan encompassing the entire Federal research effort.

It should be a plan endorsed by the scientific and industrial communities. It should be a plan that will address these initiatives and also provide for basic research and improved science education.

We propose, therefore, the establishment of a national commission. The commission would be charged to produce a national science agenda. The application of the criteria and the procedure we recommend could lead to a set of recommendations in priority order that would constitute a national science agenda. It would be a set of recommendations to execute a selection of large-scale scientific initiatives in an optimum sequence, combined with advice on the continued support of fundamental research.

It would be an agenda responsive to social and practical issues that must be considered by legislative and executive branches of Government.

The participation of the scientific and technological communities would be required. Perhaps the National Research Council, the National Academies of Science and Engineering, and the Institute of Medicine might be called upon to organize such a commission.

In any case, the relevant disciplinary societies, the American Association for the Advancement of Science, Sigma Xi, and university scientific consortia could all contribute representatives to the commission.

It would also require the appointment of leaders from business and industry and the appointment of administrators from Federal agencies supporting research.

It might be feasible and proper to have staff members from the Congress and the President's office to serve as liaison to the respective branches.

We believe that the work of such a commission would produce substantial benefits. It would contribute to the public understanding of science. It would create enthusiasm in young Americans for careers in science and engineering.

But most important, it would tell Congress and the Administration what the commission thought we ought to do, and it would tell us in what order we ought to do it. In other words, it would provide the scientific advice now lacking.

The commission should be composed of the best minds in science, technology, and science policy. Perhaps even an ethicist could be appointed, somebody informed in science and public policy that could argue for a balance between scientific interest and the public good. We are proposing the mobilization of the entire scientific community in the national interest. We are asking scientists to put aside parochialism and narrow disciplinary concerns to support an optimum agenda for human betterment and the investment of knowledge.

Thank you, Mr. Chairman.

[The prepared statement of Dr. Crowe follows:]

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TESTIMONY TO BE PRESENTED TO THE SUBCOMMITTEE ON SCIENCE, RESEARCH AND TECHNOLOGY

HEARING ON THE ADEQUACY, DIRECTION AND PRIORITIES FOR THE AMERICAN SCIENCE AND TECHNOLOGY EFFORT

FEBRUARY 28, 1989

BY LAWSON CROWE
PROFESSOR OF PHILOSOPHY
UNIVERSITY OF COLORADO
BOULDER, COLORADO

INTRODUCTION

In a recent issue of American Scientist, Dr. John A. Dutton and I published an article presenting a formal procedure for setting priorities among major scientific initiatives. [John A. Dutton and Lawson Crowe, "Setting Priorities Among Scientific Initiatives" (1988), American Scientist , Nov.-Dec., Vol. 76, pp. 599-603. A reprint of this article is attached for inclusion in the Hearing record.]

We take note of the stunning opportunities for the expansion of human knowledge represented by these initiatives, of their extraordinary scope and cost, and the liklihood that there will not be sufficient funds to undertake them all. As have others, we call for the scientific community to accept responsibility for proposing a coherent, balanced, and fiscally realistic national science agenda that would set priorities among the proposed initiatives. Such an agenda would enable Congress, the federal agencies supporting research, and the President to distinguish between those programs which appear to be mandatory, those which might offer grand new intellectual vistas or significant expansion of technological capabilities, and those on-going activities essential to the support and strenthening of American science.

Much has been said about the reluctance of scientists to evaluate activities outside their disciplines, and also how, at least in some cases, scientists prefer not to assess the possible social benefits likely to accrue from their research. Nevertheless, we argue that criteria can be established which make evaluation by the scientific community of the major proposed scientific initiatives feasible and effective. Thus we offer a rationale, a set of evaluative criteria, and a methodology for their application. It is important to recognize that this procedure is designed specifically to evaluate and place in priority order large-scale multidisciplinary programs requiring substantial funds sustained over a significant period of time.

RATIONALE FOR THE EVALUATION OF SCIENTIFIC INITIATIVES

Scientific merit, possible benefits to society, and programmatic implications are the three categories of criteria necessary for the evaluation of scientific initiatives.

Scientific Merit

Within a discipline, the concept of scientific merit is the primary consideration in justifying a particular proposal. Programmatic issues such as cost, avaliability of the requisite technology, facilities, and trained personnel are usually subsumed into the evaluation of a particular proposal's scientific merit. Evaluators may or may not attempt to assess anticipated social benefits.

In the context of evaluating major scientific initiatives, scientific merit refers to the relative potential contribution of an initiative to improved scientific understanding and perhaps control of the physical or biological environment. Weinberg [1] has pointed out that the view from within and without a discipline may be quite different. An initiative may contribute only to the originating discipline or may contribute simultaneously to several scientific disciplines. He thus proposes: "...that field has the most scientific merit which contributes most heavily to and illuminates most brightly its neighboring scientific disciplines."

In general scientific research involves three stages: exploration and discovery, reconnaissance and observation, and ultimately the development of theory, modeling and simulation. In this last stage, scientists attempt to reconcile theoretical predictions with their observations. At a particular time in the evolution of a discipline one stage or another may be the most meritorious, but each stage is of potentially equal merit. Thus we propose a working definition of scientific merit, namely, that scientific endeavors are meritorious in proportion to the extent that they reveal the laws and interactions governing the structure and evolution of physical or biological phenomena; the broader the scope of a scientific law or theory, the more universal and valuable it is.

Social Benefits

Virtually every large scale scientific initiative now under consideration contains the potentiality for substantial benefits to society. Such benefits range from improved international relations to various technological applications. Justification for the use of public funds lies in the expectation that the outcomes will produce substantial public good. In fact, it is important to recognize the public character of this type of scientific endeavor. The scientific enterprise does not exist for the entertainment of scientists. Rather, it is an activity generated by human curiosity about the world in general and our need to control and cooperate with our physical and social environment for our mutual benefit. One of the most attractive features about these monumental efforts is that they provide a sense of purpose, identity, and direction for the whole society. Although the following list of values is not exhaustive, it suggests the range of benefits that might be considered.

1) Science reveals the effects of human activities and permits us to ascertain our choices and options relative to our physical environment.

2) Scientific and technological advancement may directly support and strengthen valuable human activities, e.g. economic development, health care, national defense. It also creates technological infrastructures that support travel, communications, and computation.

3) Science contributes directly to providing more efficiently for our physical needs. It produces the leisure and abundance necessary to improve society, preserve human freedom and dignity, and pursue rewarding cultural and recreational activities.

4) Science satisfies and stimulates human curiosity. Its success creates pride in human intellectual accomplishments and reinforces national prestige.

5) Scientific initiatives provide a focus for collaboration with other nations enhancing international cooperation and good will. The international character of scientific research is a positive force for mutual understanding among nations.

Programmatic Implications

Any large scale scientific initiative will consume a major portion of the available resources for a considerable period of time. As always, the cost of foregoing other research opportunities, including support for basic research by individual investigators, must be weighed against anticipated benefits of the proposed initiative. Evaluation of an initiative must take account of the following considerations.

1) Readiness to execute the scientific program entails its technological feasibility; commitment of a sufficient number of qualified scientists, technicians, and support staff to carry out the project; adequate institutional and administrative arrangements; and concrete plans for data acquisition, analysis, utilization and archiving.

2) The impact of a proposed initiative on research and education, federal laboratories, universities, industrial con-

tractors, and international scientific collaboration must be assessed.

3) Direct, marginal, and hidden costs (as well as the opportunity costs mentioned above) must be assessed over the life of the initiative. If results of the initiative may eliminate need for some current activities, the potential savings should be calculated.

Further Reasons for a Formal, Standard Evaluation Procedure

We believe that the formal use of explicit evaluation criteria at the highest scientific decision-making level will have significant advantages over the present less structured proceedings. Proposers will be required to produce answers to a standard set of questions and will have to address the same issues. Although the rhetorical abilities of proposers may vary, the relatively homogenous documentation produced will facilitate meaningful comparisons. It should also be possible to discuss and keep separate the different weights reviewers are likely to give to the three categories of criteria. This may be of great importance since the emphasis may shift from scientific merit toward social benefits and broad programmatic issues as the proposals move to higher levels in the federal decision process.

Various numerical procedures to score proposals or to rank order them with respect to each broad category of criteria and thereby make explicit the judgments of evaluators may be employed. In numerical scoring schemes, a maximum number of points is assigned in advance to each criterion. Evaluators award points to each item in the list. Weights applied to the three broad categories then are used to produce sums and a final evaluation of the competing proposals. In rank ordering, the evaluators independently list the proposals in order in each of the three main categories. When weights are assigned and applied, the rank orders can be combined into final numerical evaluations.

The application of a formal evaluation procedure depends on the informed judgment of the evaluators. It diminishes the effect of hidden agendas and covert self-interest often present in more informal procedures. Some scientists object that numerical evaluation procedures are dangerous. They argue that numerical ratings may cause a less scientifically meritorious proposal to be selected owing to its strength in the other two broad criteria. This is tantamount to claiming that evaluators should consider scientific merit alone. Although scientific merit is a necessary condition for accepting a proposal, we do not believe that it should be the only consideration. When two or more competing proposals have (as often happens) nearly equal scientific merit but one promises more favorable social benefits or is superior in its programmatic arrangements, these aspects need to be made explicit in the evaluation process. We believe that initiatives scoring highest in all three categories are the ones most likely to be attractive at national decision levels.

The procedure we present is designed to permit a scientific advisory body to compare a set of large-scale initiatives and to reach an unambiguous set of priority recommendations. Evaluations performed in this formal and explicit manner will have the greatest usefulness for the non-scientist decision-makers in the federal agencies, Congress, and the Administration. We believe that this procedure will improve the quality of scientific advice now recognized as lacking by both scientists and members of the federal establishment. It will also encourage our orderly progression through the magnificent scientific agenda lying before us.

A FORMAL EVALUATION PROCEDURE

With the foregoing considerations in mind, we propose the specific set of criteria below for the evaluation of large-scale scientific initiatives based on the three categories of scientific merit, social benefits, and programmatic implications. The criteria are phrased as questions to be answered in formal written proposals that will be evaluated and compared as part of the priority ordering process.

Criteria for Evaluation

Scientific Merit

1. Scientific objectives and significance

What are the key scientific issues addressed by the initiative?

Why are these issues significant in the context of science?

To what extent is the initiative expected to resolve them?

2. Breadth of interest

Why is the initiative important or critical to the discipline proposing it?

What impact will the science involved have on other disciplines?

Is there a potential for closing a major gap in knowledge either within a discipline or in areas separating disciplines?

3. Potential for new discoveries and understanding

Will the initiative provide powerful new techniques for probing nature?

What advances beyond previous measurements can be expected with respect to accuracy, sensitivity, comprehensiveness, and spectral or dynamic range?

Is there a potential for insight into previously unknown phenomena, processes or interactions?

Will the initiative answer fundamental questions or stimulate theoretical understanding of fundamental structures or processes related to the origins and evolution of the universe, the solar system, the planet Earth, or life on Earth?

In what ways will the initiative advance the understanding of widely occurring natural processes and stimulate modeling and theoretical description of these processes?

Is there potential for discovering new laws of science, new interpretations of laws, or new theories concerning fundamental processes?

4. Uniqueness

What are the special reasons for proposing the initiative? Could the desired knowledge be obtained in other ways? Is a special time schedule necessary for performing the initiative?

Social Benefits

1. Contribution to scientific awareness or improvement of the human condition

Are the goals of the initiative related to broader public objectives such as human welfare, economic growth, or national security? Will the results assist us in planning for the future?

What is the potential for stimulating technological developments that have application beyond this particular initiative?

Will the initiative contribute towards public understanding of the physical world and appreciation of the goals and accomplishments of science?

2. Contribution to international understanding

Will the initiative contribute to international collaboration and understanding?

Does the initiative have any aspects requiring special sensitivity to the concerns of other nations?

3. Contribution to national pride and prestige

How will the initiative contribute to national pride and to the image of the United States as a scientific and technological leader?

Will the initiative contribute to public pride because of the magnitude of the challenge, the excitement of the endeavor, or the nature of the results?

Programmatic Concerns

1. Feasibility and Readiness

Is the initiative technologically feasible?

Are new technological developments required for the success of the initiative?

Are there adequate plans and facilities to receive, process, analyze, store, distribute, and use data at the expected rate of acquisition?

Is there an adequate administrative structure to develop and operate the initiative and to stimulate optimum use of the results?

2. Scientific logistics and infrastructure

What are the long-term requirements for special facilities or field operations?

What current and long-term infrastructure is required to support the initiative and the processing and analysis of data?

3. Community commitment and readiness

Is there a community of outstanding scientists committed to the success of the initiative?

In what ways will the scientific community participate in the operation of the initiative and the analysis of the results?

4. Institutional implications

In what ways will the initiative stimulate research and education?

What opportunities and challenges will the initiative present for universities, federal laboratories, and industrial contractors?

What will be the impact of the initiative on federally sponsored science? Will new components be required? Can some current activities be curtailed if the initiative is successful?

5. International involvement

Does the initiative provide attractive opportunties for involving leading scientists or scientific teams from other countries?

Are there commitments for programmatic support from other nations or international organizations?

6. Cost of the proposed initiative

What are the total direct costs, by year?

What are the total costs, by year, to the federal budget?

What portion of the total costs will be borne by other nations?

The Procedure

Once an appropriate body of evaluators has been established, the procedure is fairly straight-forward and familiar to scientists and agency adminstrators accustomed to ranking individual scientific grant proposals. We identify seven steps in the successful application of this procedure.

1. The specific objectives and scope of the task should be determined explicitly in advance. The advisory body must have a specific charge in respect to the proposals to be evaluated and rank ordered.

2. The advisory body must determine exactly what steps are to be taken and must schedule its various tasks. Since the presentation of homogenous documentation in response to the evaluation criteria is a crucial component and the key to success, it must be clear who will provide this written material. The advisory body must determine what system of evaluation will be used once the material has been received. It must also decide what form its recommendations will take.

The advisory body must also reach agreement, in advance, on what the evaluation criteria will be. (We recommend, of course, those given above.) We have observed already that some scientists are uncomfortable with the suggestion that public support of initiatives does not rest solely on a narrow view of scientific merit. They argue that neither scientists nor program administrators are qualified to evaluate the social benefits of a proposed initiative. Nevertheless, failure to recognize the full range of criteria relevant to these large-scale initiatives will leave this task (which will certainly be accomplished one way or another) to those with different agendas, or worse, to those who may not understand the significance of the initiative or its anticipated benefits. There is no reason that scientists should not attempt in their deliberations to perceive and apply the criteria that will in fact shape national decisions about scientific initiatives. They should not relinquish the advantage to be gained by taking a broader perspective in making their recommendations. Their proposals will be more effective if these issues are addressed explicitly and convincingly.

3. A subset of the advisory body should attempt a trial run of the procedure, working through the entire process before the full advisory body meets to prepare its recommendations. The trial run may reveal areas of conflict that require reconciliation, promotes understanding of the procedure, and gives the leadership confidence that the task can actually be completed.

4. If the advance preparations have been adequate and the procedures followed by the advisory body members, the preparation of recommendations by the full body can proceed. The main issues

revealed by the documentation can be discussed as a prelude to collecting the member's independent recommendations. It can then be determined if a consensus or strong majority opinion has been reached. If the advisory body is large, separate efforts by workshop groups will permit more effective interactions between the members.

The preparation of detailed budget recommendations will require on-line capability for budget and statistical analysis so that the implications of decisions can be readily determined. Relevant forms can be prepared in advance and tested during the trial run. Management of the on-line capability and the preparation of budgets should be in the hands of experienced, professional budget and planning analysts, not members of the advisory body.

5. The advisory body must examine in detail the implications of its recommendations for current scientific research programs. Modifications in the recommendations may appear necessary and will require working through an iterative process.

6. A report of the recommendations should be prepared and made readily available to the community members affected by the recommendations.

7. The chairman or other appropriate spokesperson of the advisory body should accept responsibility for meeting with a variety of groups to explain the recommendations. If this effort is made before the report is distributed, and if it is done effectively, it will contribute to the wide endorsement of the recommendations.

Previous Applications of the Procedure

My colleague and co-author, Dr. John A. Dutton, who is Dean of the College of Earth and Mineral Sciences at the Pennsylvania State University and President of the University Corporation for Atmospheric Research Foundation, has applied successfully this evaluation procedure with two high level advisory committees. As a member of the Space and Earth Science Advisory Committee (SESAC), Dr. Dutton participated in the development and application of the evaluation criteria and procedures described above. SESAC used these criteria [2] in arriving at recommendations for candidate missions in Fiscal Year 1988. Written proposals addressing the criteria were supplied by advocates of each mission and were evaluated by subgroups of the committee. Each subgroup arrived at essentially the same recommendations.

Dr. Dutton also served as chairman of the NSF-UCAR Long-Range Planning Committee which used this procedure to develop priority recommendations for a six year, integrated, coherent program in the atmospheric sciences. This application of the procedure is particularly notable because it entailed the analysis and evaluation of substantial multidisciplinary long-term initiatives. The committee was composed of university scientists, UCAR trustees, and representatives from NOAA, NASA, DOD, and the Board on Atmospheric Science and Climate. Its findings were reviewed by appropriate committees of the National Academy of Sciences, other UCAR trustees and Member Representatives, the senior management of NSF, and other appropriate bodies. Even though the recommendations and priorities call for redirection of some scientific activities at NCAR, the assigned priorities have been favorably received and widely accepted (The Atmospheric Sciences: A Vision for 1989-1994. Report of the NSF-UCAR Long-Range Planning Committee, July, 1987. A copy of this report is presented for inclusion in the Hearing Record.)

TOWARD A NATIONAL SCIENCE AGENDA

As suggested above, we believe that there are three major groups of scientific initiatives about which priority decisions must be made. These are mandatory efforts related to health, environment and national defense, grand initiatives offering revolutionary new intellectual vistas or significant expansion of our technological capabilities, and incremental efforts that are part of the essential advance of science. In addition, these activities must be balanced against essential efforts in basic and applied research, graduate education and the recruitment of young Americans into scientific and technological fields.

Mandatory efforts requiring suitable priority in a national science agenda are the comprehensive study of global climate changes induced by human activities, the development of improved and alternate sources of energy, the conquest of diseases such as AIDS, and the constant improvement of medical knowledge and health care for all Americans.

Grand initiatives such as the establishment of a permanent lunar base, a manned mission to Mars, the development of high temperature superconductivity that would permit revolutions in communications, computers, transmission of energy, and transportation, astronomical observatories in space, the development of vastly more powerful computer systems and the expansion of artificial intelligence providing for their more effective use are all under active study.

Incremental initiatives are the superconducting supercollider, unmanned planetary explorations, human genome mapping, the space station complex, and the national aerospace plane. All of these projects are worthy of consideration, and all of them have advocates in the scientific and technological community, the Congress, and the Administration. It seems apparent that the nation needs to establish a long-term plan encompassing the entire federal research program and endorsed by the scientific community that will address these initiatives and at the same time provide for basic research and improved science education.

We propose, therefore, the establishment of a national commission that will have as its charge the production of a national science agenda. In order to guarantee the participation of the scientific and technological community in the production of a national science agenda, the National Research Council of the National Academies of Science and Engineering and the Institute of Medicine might be called upon to organize such a commission. In any case, these bodies along with the relevant disciplinary societies, the American Association for the Advancement of Science, Sigma Xi, and university scientific consortia could all be called upon to provide representatives to the commission. In addition, leaders in business and industry and adminstrators of federal agencies supporting research should be appointed to the commission. Perhaps Congressional and Presidential staff members should also be appointed as liason to their respective government branches.

Application of the criteria and procedure described above could lead to a set of recommendations in priority order. These recommendations would constitute a national science agenda that would advocate the execution of a selection of large-scale scientific initiatives in an optimum sequence combined with advice on the continued support of fundamental research. It would be an agenda responsive to the social and practical issues that must be considered by the legislative and executives branches of government.

A national science agenda formulated and endorsed by the scientific and technological community, business leaders in science and technology, and representatives of government would contribute to public understanding and support and would create enthusiasm in young Americans for careers in scientific and technological fields. It could be expected to have significant consequences extending well into the next century. Most importantly, however, it would tell Congress and the Administration what the commission thought we ought to do and in what order. In short, it would provide the advice now lacking.

To achieve this outcome, the commission should be composed of the best minds in science, technology, and science policy, and, if I may be pardoned for saying so, a disinterested ethicist or two, informed in science and public policy, who could argue for a balance between scientific interests and the public good. Although it is true that some members of the scientific community are inclined to disciplinary parochialism and special pleading, it is our view that most scientists are informed citizens responsive to national needs. Dr. Dutton and I are proposing the mobilization of the entire scientific community in the national interest. We believe that it is time to appeal to the national interest, and, indeed, to the interest of all humankind and to ask scientists as a community to put aside their narrow disciplinary concerns and to support an optimum agenda for human betterment and the advancement of human knowledge.

Thank you, Mr. Chairman and members of the Subcommittee for the opportunity to express these views.

1. A.M. Weinberg. 1963. Criteria for Scientific Choice. Minerva 1:159-71.

2. Space and Earth Science Advisory Committee,. NASA Advisory Council. 1986. The Crisis in Space and Earth Science: A Time for a New Commitment. Washington, D.C.

BIOGRAPHICAL SKETCH

LAWSON CROWE is Professor of Philosophy in the Department of Philosophy and Fellow of the Institute for Behavioral Genetics of the University of Colorado at Boulder, Colorado. He is former Chancellor of the University of Colorado at Boulder and also served that campus as Provost, Vice President for Research, and Dean of the Graduate School. His primary field of interest is Applied Ethics. He has contributed articles to professional journals on ethical issues in medicine, alcohol research, behavior genetics, information technology, and science policy. He teaches undergraduate and graduate courses as a member of the Center for the Study of Values and Social Policy of the Department of Philosophy.

Mr. **Walgren**. Thank you very much, Dr. Crowe.

Somehow I can only be reminded of the talk show I was listening to last night out of Boston, where the fellow had on two of the best advocates of building new houses through the HUD program or new housing through the HUD program versus feeling that there was a surfeit of housing at present and trying to work on its distribution.

It was an extremely interesting program, and I have the same reaction to hearing several of you lay out where you think we are.

I want to say at the outset that I think there is more meat in these statements, all of them, than I can get my own mind around. I look forward to going back to them and trying to deal with them.

Let me ask, I think, Dr. Cyert, you indicated that it goes back to the Eisenhower years before you find an effort—or was it Dr. Branscomb; I can't remember—an effort to set out the objectives, and your thought of a national commission of some kind that would do that for us.

Does it really go back that far? What do we have in that field now?

Dr. **Cyert**. To the best of my knowledge, that is true. There has not been an attempt.

I don't know if that was an overwhelming success—but there was a real attempt there to try to develop a set of national objectives, and there was such a report, and I believe that was the last time we tried to do it on a national level.

Mr. **Walgren**. Have there been other efforts by the various national science bodies that we have to set out in a comprehensive way objectives, national objectives?

Dr. **Cyert**. Well, I think that to some extent each of the bodies—NSF and NIH I know best—I think each of them does have a set of objectives partially coming from Congress for various programs that they have. I don't think that there is an attempt, say, to have a set of overall objectives that are really guiding for either NSF or NIH.

I think there are some general sets of objectives, but I think it really gets divided up into the programs and then the various programs have some objectives.

Mr. **Walgren**. I guess what I am curious about is that we ought to, as a staff on our committee, we ought to go back and look at what was done under Eisenhower and then see what other efforts might have been made in that same area.

Dr. Branscomb?

Dr. **Branscomb**. I would like to suggest that it may be helpful to try to separate out in one's thinking the 12 percent of the national R&D effort which is devoted to fundamental science from the other 88 percent. In the case of the fundamental science, there are a lot of other places you can go to find very helpful work on analysis of opportunities, indeed setting of priorities at least within broadly defined disciplines.

The Committee on Science and Public Policy of the Academy has over many years done that kind of planning. I think at the aggregate level in basic science probably it hasn't been done, although I remember in the late '60s the President's science advisory committee had a panel under Harvey Brooks which did an 8000-page study of basic science which attempted to look at the whole spectrum.

That task, I believe, one can make progress against it, and Frank Press' speech last year suggested that that kind of effort might be worth making. But I believe that much of the motivation for the request of the budget committee to the academies for the report we are discussing today, and perhaps even motivation for this hearing, has to do with the totality of at least the Federal portion of the national R&D, most of which most of the technology and the research is the servant of a set of objectives which are not scientific objectives.

My skepticism about Professor Crowe's suggestion is that while I do not disagree at all with his criteria, I am very nervous about asking the scientific community to make judgments which in the normal course of events our representatives in Congress and our elected officials in the Government are supposed to make about the relative importance of the Federal role in bolstering the economy versus defending ourselves against potential hostile Nations, versus improving the public health and many other objectives.

Mr. Walgren. If you were to limit yourself to the fundamental research, do you have the same reservation about scientists looking across the board and projecting a value, essentially a comparative value?

Dr. Branscomb. No, I don't, and I believe that problem is not as difficult intellectually as it has often been painted to be by scientists writing about this subject. It is difficult politically for scientists to get together and decide that this field should get more and that field should get less.

But if there were a decision process that had any muscle in it so that priority decisions would be really reflected in budgets, then I believe that those decisions can be made.

And in fact that is why we ought to have a strong Office of Science and Technology Policy, because they ought to have the monkey on their back to make those kinds of decisions. We don't have the balance between the fields of science that would make sense from an intrinsic point of view.

Dr. Cyert. I would go along a great distance with Lew. I think the notion that I was pushing for, this Delphi technique which would try to give you some better notion on a scientific basis of where there would be breakthroughs and where progress of a particular kind could be made where you could have a series of different criteria applied to the survey, would give you evidence along with a set of national objectives on how you wanted to allocate. And you would not necessarily be bound by somebody else who said these are the things that are the most important.

It is in a sense what I am talking about as another way, I think, of doing what Professor Crowe is talking about, without making it quite as finite.

Mr. Walgren. Dr. Crowe?

Dr. Crowe. Mr. Chairman, I want to reply to Dr. Branscomb. I did not hear Congress asking the scientists to make the decisions. I heard Congress asking scientists for advice on sequence and priority and ordering of decisions. And it seems obvious that between the very large-scale initiatives that are being proposed, some are more ready to go than others, all of them probably should be done at one point or another, some are more ready to go than others, some have greater possibility of benefits of one kind or another, some will have external criteria that will be more significant for the society in general than others.

My own view is that public money should be spent for the public good and not simply because something needs to be done. It may well be the case that you want to do something later on but you may have urgent priorities. And what I heard the hearing to be about was how you decide what those priorities ought to be. And it seems to me the scientific community is as well qualified to suggest, presumably again in the national interest, what the priorities should be, and that is in what sequence things should be taken up as any other body. And if it declines to do so, then somebody else is going to get the agenda and it may be indeed not the best sequence that we could have.

Mr. **Walgren**. It goes into an area where there has been a little tension between the Congress and the scientific community where to a certain degree the science community wants to be able to say what should be done and they want to decide what it is to do. And there has always been a little resistance, and some of us on the other side who I think go a little bit more toward your description of seeking advice that feeling that the decision as to what to do ought to be retained more closely on the congressional side.

Let me just ask, you mentioned UCAR and how that seemed to be a successful comparative effort in that area. How would these big projects, even though they might all—should be—done sometime—how do those big projects that we think of—space station and supercollider—fare in your criteria evaluation at present?

Dr. **Crowe**. In our discussion we have divided the various projects that are proposed into three categories. It seems to us that some are things that we really need to do. The improvement of health, for example, dealing with the prevention and cure of AIDS is something that needs high priority, in our judgment. It is almost mandatory, if not actually mandatory.

There are other projects that have the capacity to expand the human consciousness eventually; that is, that would give us a new vision of the universe and that sort of thing, tell us the secrets of the universe. The issue with them is when do you do them? That is, at what point do you have sufficient resources to do them and do them well?

And then there is sort of incremental things that just need to be done to strengthen science and so on.

And we have divided into those three categories. My own sense is that some of these initiatives, as proposed, are more ready for exploitation and for implementation than others. Some are rather more long range. They involve—they ought to involve, it seems to me—by the scientific community a lot of thought and planning as to what sequence of events should occur, because for one thing, when you do scientific experiments that have to do with interplanetary exploration, unmanned exploration and that sort of thing, you are dedicating entire careers to these projects.

That is, they take so long to receive the data, to evaluate them and so on, that many people have to make long-range commitments in order to participate in them.

And for that reason, I think if you were to lay out all of these proposals side by side and evaluate them in the light of the broad criteria that we have proposed, and some of those that are in Dr. Branscomb's testimony, that you would see that some are quite different from others. And in seeing that, you would know in which order to undertake them and the scientists themselves would know it.

I have to say that I think all of my experience has been that most scientists are reluctant to do this, but if we seek advice from them, what else can we ask them to do but that?

Mr. WALGREN. Mr. Boehlert?

Mr. BOEHLERT. Dr. Crowe, I commend you for your work. And I must confess—and I plead guilty to this charge—I think Congress is a collection of scientific illiterates, and that should surprise no one because we sort of mirror the American public. So, when we reach out and look to people like you, I would agree with your analysis that we are asking for the best counsel and the best advice, the best direction we can possibly get.

We are called to make judgments on things like the superconducting supercollider that, with the exception of George Brown, I doubt if anyone in the Congress really understands. It is difficult.

But I am wondering now, if you established the criteria, would you give equal weight, for example, to contribution to international understanding that you would give to national pride and prestige? It's a tough thing to do, isn't it?

Dr. CROWE. It is difficult. But what we have done with these two evaluations that we conducted is we allowed the people who were doing the evaluation to decide the weights that they would give to the categories. And in the case of the UCAR long-range—NSF-UCAR long-range planning committee—that committee gave twice the weight to scientific merit as it did to the other two broad categories. They did not try to weight every question within the broad categories, but then they ranked them independently, having decided in what order they should rank them in terms of scientific merit and they ranked them according to the other two categories. They summed those scores. They did use a numerical evaluation system.

And they found almost from the outset unanimous understanding of the order in which these activities should be supported. And they were dealing with highly competitive proposals in the atmospheric sciences.

Dr. Dutton's report, which I have transmitted to the committee, contains the details of that evaluation process.

I think there is more unanimity among scientists than scientists tend to admit to themselves. There is more sense of what ought to be done than they are likely to say, because they have the same respect for others. They have been trained not to make excessive claims for their work, and that is a very rational and prudent thing to do.

But I think there is more unanimity there than you would imagine.

Mr. **Boehlert**. I imagine you have heard a great deal from your colleagues in the scientific community. What is the general reaction to your proposal?

Dr. **Crowe**. I think the skepticism that Dr. Branscomb expressed is pretty generally a reaction. There is also the fear that something like centralized decisionmaking might issue from such activity.

The model I have in mind is the President's commission on biomedical ethics, which met about a decade ago and produced a report that has had a profound impact on the way that health care is delivered in this country. It has resolved many of the ethical issues, or at least it has raised them up to the surface where they can be considered. And it just has had, I believe, a significant impact on the consciousness of people delivering health care in this country.

I think a national commission could have a similar profound impact on the way in which we view science and the way in which the public understands it and the way in which we formulate its objectives.

Mr. **Boehlert**. Thank you.

The title of this hearing is the "Adequacy, Direction, and Priorities for the American Science and Technology Effort." Let's begin at the beginning.

Dr. Branscomb, you talked in your testimony, your submitted testimony at page five, about education, and you say the math and science problems in the U.S. K through 12 education are not only serious problems, "their scale is enormous."

That is probably understatement, is it not? It is in crisis. Are we not a Nation, a Nation at risk? I know that is overused, but is it not of crisis proportions?

I was reading something just recently that said that more than half the kids in the New York City school system will graduate from 12th grade without ever having firsthand exposure to a certified science teacher.

We probably have more history majors teaching science than we have science majors.

Dr. **Branscomb**. 48 percent of the cohort will not graduate at all, even from a school with no science teachers.

Mr. **Boehlert**. Where do we start? What can we do in this area that you would recommend?

For example, I have tried to make a modest contribution, and I will shortly introduce the bill along with the chairman, who has some legislation that would set up a grant program for college students who major in science and math and agree to teach in a public education system.

Dr. **Branscomb**. Let me say I think the place the Congress could make an absolutely crucial investment—and I think it is an issue before the Congress this year—is the Federal participation for a limited period of time on a matching basis with the National Board for Professional Teaching Standards.

This is a group whose board of directors now consists of a strong majority of active classroom teachers, but it grew out of the Carnegie Corporation's study task force which I chaired on teaching as a profession, which envisions a major reform in the public school system, really deregulating the schools, putting education back in the hands of teachers, and then trying to make sure that we have qualified teachers, with a variety of changes in teacher education that go with that.

The National Standards Board would have the teachers set the standard, and they would have a voluntary board examination that they would give to their own members. And in the course of the research to assure that we will learn—a great deal of very fine university level research is being done. The benefit of that would be to learn—a lot more about how good teaching is done.

This is a specific case where there is by a consensus of the education community and the Governors' Association, which unanimously adopted these recommendations, a welcomed participation by the federal government.

Mr. **Boehlert**. One further question, totally off in a different tangent.

I have always been laboring under the conception that one of the reasons why the Japanese are doing so well in the international marketplace is that the federal government over there has been really very generous in supporting industrial research, and I thought they were doing a great job and we were not doing a good job at all, and then I read a National Science Foundation report just recently that indicated that only 2 percent of the industrial research in Japan is financed by the federal government; whereas, 35 percent of the industrial research in the United States is financed by Uncle Sam.

Dr. **Branscomb**. That is true, but two words of explanation.

One is that in Japan, under a special tax provision statute, the companies establish in some cases private foundations that are set up by the company and exist for the purpose of doing research in collaboration with the government on government projects.

So there are institutions in Japan that are doing research in connection with companies that are not themselves part of the company's budget and for this reason do not show up in the statistics. So the 2 percent would be a little bit bigger, but not much.

On the U.S. side, because in fact in Japan 80 percent of national R&D is performed in industry and paid for by industry (whereas, in this country it is 50 percent), those ratios give the result that I quoted earlier, that their companies spend 50 percent more per unit of output on R&D than our companies.

On the other side, of course, the reason why 35 percent of American industrial R&D is paid for by the government is that it is all military, virtually all military. That is about the percentage, since we do 73 percent of our R&D in our companies but only 50 percent is funded by the companies. That difference is now almost entirely military since the big energy projects have been—the big civil development programs have virtually all been wiped out except for FAA.[2]

Mr. **Boehlert**. So if you draw a logical conclusion from what you are saying—I am not sure I am logical—in Japan the government really is doing more to support industrial research than we are in the United States?

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[2] Dr. Branscomb indicated that "NASA" and "DOE" should be added to "FAA" for completeness.

Dr. **Branscomb**. The curious thing is the figures would suggest that the government has a significant role. That is in part their Ministry of Education funds their universities, and every professor's salary is assumed to be contributing to research.

So the figures are a little bit deceiving, but I think the important issue is that MITI, which Dr. Cyert referred to earlier, has been an effective and prudent agency. It does in fact support selective projects through industry consortia in these institutions I was describing aimed at critical and strategic technologies. But MITI is very careful to fund as little as they possibly can. They use their money with a lot of leverage.

If you look at the dollars spent, with a few glaring exceptions, they are not terribly big, but they get a lot of consequence. And, furthermore, much more important is that MITI also is the coordinating body within the government for trying to get coherent national policy on trade issues, on economy strategies, and on other issues that relate to their international trade.

So if you read Prestowitz' book, *Trading Places,* you may not agree with all of his views, but you have to be impressed with the fact that when he found himself trying to negotiate trade problems with the Japanese his problem was that the Justice Department, the Treasury Department, the State Department, the Defense Department and the Commerce Department were all at each others' throats and he never had a coherent U.S. position with which to deal.

So the piece of MITI that involves trying to aggregate the smarts in new and emerging areas and get people working together, that President Cyert referred to, that is important, and I like his approach much better for America than the MITI approach. That still leaves the other piece of MITI to answer for, and that we have to do with more coherent national technology and trade policy.

Mr. **Boehlert**. To aggregate the smarts and the private sector, do we have to do something with our antitrust laws?

Dr. **Branscomb**. I think we have to be sure that there is consistent interpretation of the antitrust laws throughout all of the places where they apply. The Justice Department has been quite forthcoming, as best I can tell, on issues of cooperative R&D.

The fact that in '84 you gentlemen passed a law helps quite a bit. And yet the Justice Department's intervention in the semiconductor dumping process to prevent the U.S. side from asking for the data and for minimum pricing to prevent dumping was done on antitrust grounds. And essentially the Justice Department said you folks are trying to create a cartel in Japan and that is illegal; whereas, the cartel existed and the Commerce Department was just trying to get them to charge what it cost them to produce the products.

Mr. **Boehlert**. Thank you.

Thank you, Mr. Chairman.

Mr. **Walgren**. Thank you, Mr. Boehlert.

Mr. Brown.

Mr. **Brown**. I have no questions, Mr. Chairman.

Mr. **Walgren**. Mr. Skaggs.

Mr. **Skaggs**. Thank you, Mr. Chairman.

I would like to make a special welcome to Dr. Crowe, who I am pleased is a constituent of mine and is before the committee this morning.

There is something, I guess, seductive about a kind of general theory of relativity for integrating all of these considerations, and so I am subject to being seduced by it.

I guess I would appreciate the further observations by Dr. Cyert and Dr. Branscomb about whether we should yield to or avoid the seductivity of Dr. Crowe's proposal of trying to figure out both a theory and a structure to apply the theory to help presumably us and appropriate people in the Administration decide how to spend our money.

Dr. **Cyert**. I would say that I—the suggestion I was making, which was really utilizing some knowledge that the Japanese make—that I am trying to do in a different way but come along with the same objective that I think Professor Crowe was talking about and I think in a much more structured and rigid way. I think we should have the input from the scientific community. I think it should be done in a way that is looking forward, that is doing technological forecasting and is a technique that brings about some focus on these various forecasts.

But the Japanese, for example, use something like this. They asked in June of '86 3142 people to make forecasts over the 30-year period. That ranged until 2015, and they had six categories to keep in mind—degree of importance, time of realization, limitations and realization or inhibitions causing a nonrealization, methods to promote research and development, agents for research and development, national policies and measures.

And then they did a second round feeding back the results, which is the Delphi technique, to bring about increased focus.

Out of this, then, they got a series of fields, each with a certain number of subjects, and had some ranking on degree of importance of these fields. And I am saying that rather than necessarily asking the scientists which ones should be done and in which order—what order—to leave that kind of decision to the Congress, but give them information that would enable them to make an informed decision.

And I think getting this kind of a survey, getting this kind of a focus would give you the kind of information which would make it very possible to do. So that what I am talking about is really closely relating to what is proposed, that it is a different method. And I am saying join that together with a set of national objectives so it will enable you to say, okay, which of these things then that scientists think are important do we think are important in relationship to the national objectives that we want to achieve.

But I think either one would be an improvement on what you have to do now, where you are basically making decisions without an adequate amount of information.

Dr. **Branscomb**. Let me try that one, too.

I don't have a quarrel with a utility with the kind of information that comes from this sort of Delphi study. I would remind you, however, that what you get from that sort of study is the collective iterative judgment of the people who participate in it, and when you decide who they will be you have determined something about the answer.

First of all, if you use a lot of people, then to some extent the answers you are going to get back reflect the status quo, and the power of science is to disrupt the status quo, so that may not be the information you want.

It is like the weather. The best way to predict the weather is to forecast for tomorrow the same weather as today. It is highly correlative, but that is dull and uninteresting. You want to know if it is going to be startingly different tomorrow.

It makes a difference what the mix of the doers and users of research you use because engineers have a role in this, too, and they are by and large the consumers and the producers of goods and artifacts and capabilities. I think users need an important voice in these kinds of considerations.

But I would also urge that you listen very carefully to Dr. Richard Muller, whose testimony I quickly read, although he has not given it yet. And if the spirit of his oral testimony reads like the written one, then I don't agree with everything he is going to say but I sure agree with the fundamental premise, which is that it is the quality of technical judgment by the most imaginative and best informed people that is going to give us the best science. And the magic of our science is that we have over a long period of time not only decentralized the initiative to make proposals and try to get funded, we have also decentralized in the agencies the management of that process.

If one agency has poor technical judgment, there is always the chance you can get it from another. The system doesn't work very well, as Dr. Muller points out, but it is a lot better than trying to make those allocations in science through a set of central priorities established through panels of wise people.

And let me add two more thoughts.

There is the other end of the problem, and I will go back to what Dr. White said, which I thoroughly agreed with. I happen not to be a great enthusiast for the space station if I evaluate it as a piece of engineering investment. Now, what are we going to learn from $30 billion, or whatever it is, in the space station? And I suspect we could probably do it better and learn more but that is not the issue.

The issue is, as he pointed out, is the United States going to have a manned space program or isn't it. And if it is, it needs a space station and a cheap, small, efficient modern Shuttle, which it doesn't have. And we should be restructuring the whole space program over the next 25 years to get it back structured like it ought to be for doing manned space exploration.

You cannot escape making that judgment. Either this country is going to decide to drive a stake in the ground on manned space or it is not. In recent years it didn't. It is going to have to face up to it and pay the piper or not pay the piper. If you want to pay the piper, then you are going to have to have a space station. It doesn't make any difference whether the scientists tell you they are going to learn a lot.

I think zero gravity research is a bummer. There is very little we would learn from it. That is not the reason for building a space station. It is because this nation wants man in space.

So that set of questions you cannot escape. You do need the best technical judgment about how to do it cheap, how to do it with minimum risk, how to do it so you learn as fast as you can.

Then there is this whole middle range, and in the middle range, which mostly is the useful research that is done often in collaboration between universities and industry, on which economic progress depends. And there—I don't think you can sit back and set priorities by abstract scientific judgments because the market judgments, the engineering judgments, the industry strategy which the government doesn't determine and doesn't want to determine have to drive that. And in that area you have got to set up processes for making decisions that the government doesn't attempt to control too tightly but does have a real voice in.

You need a collaboration between the government and industry very much through the vehicle of universities in the case of the advanced research activities. So there is no escaping it is a messy problem.

Mr. Skaggs. "Messy" was going to be the word I used in the follow-up question for Dr. Crowe.

I am messy adverse as anybody and would love somehow to neaten this thing up. I guess I would like your observations about whether—if we were to attempt to create a mechanism along the line that you suggested, what the risks are that having done it we end up with a wrong result coming out, that then we are worse off than if we just left things relatively messy.

Dr. Crowe. Well, I think much of what Dr. Branscomb says is true, but our proposal really deals with these large-scale scientific initiatives. We are not trying to centralize the decisionmaking process for all kinds of scientific research. In fact, we insist that fundamental research and improved science education are absolutely essential if we are going to continue to maintain our leadership in the world.

What we are interested in doing is helping people decide, Congress and the President's Office in particular, how to choose among initiatives that already have their advocates. Their steering committees are already formed, they are already moving ahead, appropriations have already been made to NIH, for example, for the human genome mapping and sequencing, and there is the indication that more money will be requested in the future for that project.

The superconducting super collider already has a site selection. These things are in the works, and they are probably going to happen one way or the other, one time or another, not for scientific reasons necessarily, but for the kind of political and social considerations that are mentioned here.

What we would like to see happen is some sort of mechanism, and we proposed a national commission, but some sort of mechanism to say in what order and in what way those projects should be carried out from the point of view of those who are going to execute them, from the technological point of view, and from the scientific point of view.

Some of the proposals are not really scientific proposals. They are essentially engineering proposals. If we send a man to Mars or men to Mars, it may well be the case that we won't learn very much in the scientific way, at least not about getting there. It is a way of figuring out how to do it and how to get them there and how to get them back, that kind of thing.

That is a magnificent idea, but the question is when should we do it and at what cost, and I agree with Dr. Branscomb, we need the best technical advice on this subject.

At the present time what we have is a system of advocacies. We have people who are in favor of one project or another and no one to tell us in which order we should do them or whether we should do them at all, whether they are technologically feasible, and so on.

I have read recently in a research report in science—I will not name the project. I do not want to distinguish between them here, but I read of one project that is proposed that the people who are proposing it freely admit that it has no discernible benefits to society. It will simply be an advance of knowledge.

Now, that is not bad in itself, but it is one of the things that happens to be true about this particular project.

Further, they admit that the PhD scientists are not yet available to operate it and that the facilities that are required don't yet exist. They are proposing the building of physical facilities all over the country at various research centers in order to carry out the project. None of this activity is yet in place, and the proposal is for very substantial sums of money over a period of 15 or 20 years.

Now, I think if you compared that kind of proposal with somebody that was ready to go, with somebody who had something that they could do with discernible social benefits either for medicine or health or national defense or something of that sort, that the first project wouldn't stand up very well, and we need the people who understand these things to sit together and I think evaluate them and say which in their judgments are the most important.

Not that we need to centralize all scientific decisionmaking, but that we need to deal with these major opportunities that are before us at the present time. And that is the sort of thing that I think our proposal deals with, not the whole range of industry/government relations and the whole range of technological possibilities, but just those things that we are currently facing. I think there are five or six of them, major dimensions that need to be considered together so that some priority ordering can be achieved.

Mr. SKAGGS. Thank you, Mr. Chairman.

Mr. WALGREN. Mr. Skaggs, thank you.

Any second round?

Mr. BOEHLERT. Mr. Chairman.

I just want to thank you, Dr. Crowe, once again, particularly for your last little essay.

I look at the superconducting super collider, for example, and think about that and its history on Capitol Hill. If anyone thinks that that project is going forward because there is a great outpouring of sentiment here to advance a scientific endeavor, they are wrong.

It is viewed as a massive public works/jobs project, and the minute the site was selected SSC got into trouble because one by one my colleagues who were supporting this project suddenly see the day ahead when they are no longer in the zone of consideration. So suddenly it is very expensive and it is something that we have to rethink.

So I couldn't agree more with what you just said, and I thank you for it.

Mr. **Walgren**. Well, on behalf of the committee, let me thank you all for being a resource to us. We certainly appreciate your contribution to this record and look forward to talking to you about this area personally.

[Panel excused.]

Mr. **Walgren**. Let me call the third panel, Dr. Charles Fefferman, Professor of Mathematics at Princeton; Dr. Richard Muller, Professor and Senior Scientist in the Department of Physics at the Berkeley Campus, University of California; and Dr. Pollard, Professor of Cell Biology and Anatomy at Johns Hopkins Medical School.

Welcome to our committee, gentlemen. We appreciate your coming, and maybe you want to spread yourselves out a little bit more unevenly along the table so you each have access to a microphone, and let me ask you to introduce us to your testimony in perhaps something over seven minutes or so in consideration of the time.

The bells have started ringing. There is a limit to all things. But anything that you would submit in writing will be reproduced in the record for those who use these records, and we certainly appreciate your having thought about this area and coming and sharing some insights with us.

Let me ask you to go in the order in which I introduced you to the record, and so we will start with Dr. Fefferman.

STATEMENT OF DR. CHARLES FEFFERMAN, PROFESSOR, DEPARTMENT OF MATHEMATICS, PRINCETON UNIVERSITY, PRINCETON, NEW JERSEY 08544

Dr. **Fefferman**. Mr. Chairman, members of the committee, thank you for the opportunity to appear before this subcommittee to discuss the adequacy of support for university based research and to comment on setting national priorities in science funding, especially as they relate to big versus little science.

As it happens, Mr. Chairman, the first question that you posed in your invitation to testify enables me to get rather quickly to the essence of my views. Let me read from your letter.

"Given the current budget constraints and the increasing demands of large science projects, do you feel that adequate and consistent support for small-scale scientific research, university infrastructure, and science education should receive special priority in the federal R&D budget?"

My answer to this question is a resounding "yes." Let me proceed to explain in more detail what I mean.

Small science, as practiced by the individual investigator or in small groups, is the creative center of the national research enterprise. Preserving the strength of this creative center is a matter of high priority for all of science, but for my field especially.

Mathematics is the quintessential small science. Groups of size 1 to 3 are typical in mathematical research, often assisted by one or several graduate students who learn by watching the experts and developing their own skills in the process.

In the past 20 years, U.S. mathematics has produced a tremendous diversity of first class research, advancing dramatically both the discipline and its applications to science and engineering. The core of government support in mathematics should be for individuals and small teams that produce the remarkable track record of the past generation.

To preserve the preeminent position the United States has held in mathematics research since the Second World War, there is another major area which must be addressed, and it relates to the second question that you posed; namely, the number and quality of graduate students.

We need urgently to attract more and better graduate students into mathematical sciences. The number of doctorates at U.S. universities declined steadily from its peak of about 1250 in 1970 down to about 750 in 1979. In 1988 the number stood at about 800.

We are actually concerned about three issues in PhD production:

First, current production of PhD's is inadequate. We are not renewing our research people. There is a substantial shortfall in PhD's for the academic marketplace, a shortfall destined to worsen as faculty retirements increase in the next decade.

Second, the percentage of U.S. citizens among new PhD's in the mathematical sciences has been in a steady decline since the early '70s and is not at an all-time low of 45 percent. From 1972 to 1988, the number of U.S. citizens earning PhD's in mathematical sciences decreased by 55 percent and shows no signs of turning around. We are already over-dependent on foreign graduate students.

Third, the quality of graduate students is down. We noticed a marked decline in the quality of graduate students since the mid-1970s. The decline is even more severe among U.S. citizens than among foreigners.

Federal support for graduate students and post-doctorals in mathematics has significantly increased in recent years. Our problem today is not so much finding opportunities for young people, but finding young people for the opportunities we have.

Let me now discuss the infrastructure at the universities.

Since the influx of talented young people has slowed, the support of senior investigators is now critical. As I indicated earlier, support of the individual investigator is particularly important in our people-intensive small science. Since small research grants form the core of support in our field, they are what we depend on to carry our part of the university infrastructure.

The 1984 David Report recommended that the number of senior investigators supported in mathematical sciences be increased to 2600, as part of a national plan to restore balance between the support of mathematics and the support of related fields.

In Phase I of implementing the recommendations of the David Report, no progress has been made toward increasing the number of senior investigators supported, largely because we have assigned the highest priority to increasing support for young people. That we were able to increase support at all during these tight money times is primarily due to the high priority assigned to mathematics by OSTP and by Erich Bloch, Director of the National Science Foundation.

We are hopeful that the strong support will continue during Phase II of implementing the David Report's recommendations because the mathematics infrastructure is still weak.

We believe that strengthening the infrastructure should be a high priority concern for this subcommittee and for all of science. As Donald Fuqua, the former chairman of the House Committee on Science, Space and Technology, once said, "Mathematics is a foundation science. It develops tools for all of science and engineering."

The foundational role of mathematics is particularly evident in education, through which my discipline feeds the pipeline for all of science and technology. It permeates the teaching and practice of engineering, as well as the natural and the social sciences. Mathematics teaching accounts for 10 percent of all college and university teaching hours and represents a massive effort at our primary and secondary schools.

Education plays a very special and highly important role in mathematics research departments. Mathematics faculty members carry higher teaching loads than their colleagues in other science disciplines. A higher percentage of their teaching is addressed to students majoring in other specialties.

Moreover, as a result of upcoming efforts to upgrade the preparation of our nation's secondary school science and mathematics teachers, the already formidable educational responsibility of our university mathematics faculty will further increase.

Let me respond to the question you raised about setting priorities.

As a general principle, I believe that decisions should be made at the lowest possible level. Within scientific disciplines, the peer review process has worked well. Across different disciplines, panels of distinguished scientists, such as the National Science Board, have given good advice. In setting national priorities, responsibility clearly rests with the highest levels of our government.

Decisions on huge projects, such as the Superconducting Super Collider or the Human Genome project, belong in the category of national priorities. While we scientists should be involved in the debate, it is not our role to make the final decision.

Let me now conclude my presentation.

I commend the subcommittee for its concern for the health of small-scale scientific research, the university infrastructure, and for mathematics and science education. I hope that I have been able to convey to you a sense of urgency for action to provide a stable environment for scientific research at our universities and once more to attract the best and the brightest young people to scientific careers.

Despite the small money involved, our university infrastructure in mathematics is not a small problem. It has an impact on the fundamental vitality of the mathematical research and education enterprise. The health of that enterprise affects the entire science and engineering effort because of the fundamental nature of mathematics in scientific research and education.

Thank you, Mr. Chairman, for the opportunity to testify before your subcommittee.

[The prepared statement of Dr. Fefferman follows:]

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STATEMENT OF
DR. CHARLES L. FEFFERMAN
BEFORE THE
SUBCOMMITTEE ON SCIENCE, RESEARCH AND TECHNOLOGY
OF THE
U. S. HOUSE OF REPRESENTATIVES

FEBRUARY 28, 1989

Mr. Chairman, Members of the Committee. Thank you for the opportunity to appear before this subcommittee to discuss the adequacy of support for university-based research and to comment on setting national priorities in science funding, especially as they relate to "big" versus "little" science.

As it happens, Mr. Chairman, the first question that you posed in your invitation to testify enables me to get rather quickly to the essence of my views. Let me read from your letter:

> "Given the current budget constraints and the increasing demands of large science projects, do you feel that adequate and consistent support for small-scale scientific research, university infrastructure, and science education should receive special priority in the federal R&D budget?"

My answer to this question is a resounding yes! Let me proceed to explain in more detail what I mean.

MATHEMATICS - THE QUINTESSENTIAL SMALL SCIENCE

Small science, as practiced by the individual investigator or in small groups, is the creative center of the national research enterprise. Preserving the strength of this creative center is a matter of high priority for all of science, but for my field especially.

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To preserve the preeminent position the United States has held in mathematics research since the second World War, there is another major area which must be addressed, and it relates to the second question that you posed.

NUMBER AND QUALITY OF GRADUATE STUDENTS

We need urgently to attract more and better graduate students into mathematical sciences. The number of doctorates at U.S. universities declined steadily from its peak of about 1250 in 1969-70 down to about 750 in 1978-79. In 1987-88 the number stood at about 800. We are actually concerned about three issues in Ph.D. production:

(1) Current production of Ph.D.s is inadequate

- We are not renewing our research people;

- there is a substantial shortfall in Ph.D.s for the academic marketplace -- a shortfall destined to worsen as faculty retirements increase in the next decade; and

- the shortage of school mathematics teachers is even more serious than in any other discipline.

(2) The percentage of U.S. citizens among new Ph.D.'s in the mathematical sciences has been in a steady decline since the early 1970's, and is now at an all-time low of 45%. From 1972-73 to 1987-88, the number of U.S. citizens earning Ph.D.'s in mathematical sciences decreased by 55% and shows no sign of turning around.

(3) The quality of graduate students is down.

- We notice a marked decline in the quality of graduate students since the mid 1970's; the decline is even more severe among U. S. citizens than among foreigners.

Federal support for graduate students and post doctorals in mathematics has significantly increased in recent years. Our problem today is not so much finding opportunities for young people, but finding young people for the opportunities we have.

THE INFRASTRUCTURE AT THE UNIVERSITIES

Since the influx of talented young people has slowed, the support of senior investigators is now critical. As I indicated earlier, support of the individual investigator is particularly important in our people-intensive small science. Since small research grants form the core of support in our field, they are what we depend on to carry our part of the university infrastructure.

The 1984 David Report recommended that the number of senior investigators supported in mathematical sciences be increased to 2600, as part of a national plan to restore balance between the support of mathematics and the support of related fields. In Phase I of implementing the recommendations of the David Report, no progress has been made towards increasing the number of senior investigators supported, largely because we have assigned the highest priority to increasing support for young people. That we were able to increase support at all during these tight-money times is primarily due to the high priority assigned to mathematics by OSTP and by Erich Bloch, Director of the National Science Foundation. We are hopeful that this strong support will continue during Phase II of implementing the David Report's recommendations, because the mathematics infrastructure is still weak.

We believe that strengthening the infrastructure should be a high-priority concern for this subcommittee and for all of science. As Donald Fuqua, the former chairman of the House Committee on Science, Space and Technology, once said: mathematics is a foundation science. It develops tools for all of science and engineering.

MATHEMATICS AND SCIENCE EDUCATION

The foundational role of mathematics is particularly evident in education, through which my discipline feeds the pipeline for all of science and technology. It permeates the teaching and practice of engineering, as well as the natural and the social sciences. Mathematics teaching accounts for 10% of all college and university teaching hours, and represents a massive effort at our primary and secondary schools.

Education plays a very special and highly important role in mathematics research departments. Mathematics faculty members carry higher teaching loads than their colleagues in other science disciplines. A higher percentage of their teaching is addressed to students majoring in other specialties.

Moreover, as a result of upcoming efforts to upgrade the preparation of our nation's secondary school science and mathematics teachers, the already formidable educational responsiblity of our university mathematics faculty will further increase.

SETTING PRIORITIES

As a general principle, I believe that decisions should be made at the lowest possible level. Within scientific disciplines, the peer review process has worked well. Across different disciplines, panels of distinguished scientists, such as the National Science Board, have given good advice. In setting national priorities, responsibility clearly rests with the highest levels of our government.

Decisions on huge projects, such as the Superconducting Supercollider, or the Human Genome project, belong in the category of national priorities. While we scientists should be involved in the debate, it is not our role to make the final decision.

CONCLUSION

Let me now conclude my presentation.

I commend the subcommittee for its concern for the health of small-scale scientific research, the university infrastructure, and for mathematics and science education. I hope that I have been able to convey to you a sense of urgency for action to provide a stable environment for scientific research at our universities, and once more to attract the best and the brightest young people to scientific careers.

Despite the small money involved, our university infrastructure in mathematics is not a small problem. It has an impact on the fundamental vitality of the mathematical research and education enterprise. The health of that enterprise affects the entire science and engineering effort because of the fundamental nature of mathematics in scientific research and education.

Thank you, Mr. Chairman, for the opportunity to testify before your subcommittee.

Mr. WALGREN. Thank you, Dr. Fefferman. We appreciate that. Dr. Muller.

STATEMENT OF DR. RICHARD MULLER, PROFESSOR AND FACULTY SENIOR SCIENTIST, DEPARTMENT OF PHYSICS, LAWRENCE BERKELEY LABORATORY, UNIVERSITY OF CALIFORNIA, BERKELEY, CALIFORNIA 94720

Dr. MULLER. I apologize in advance for not giving more polished testimony. Yesterday morning I was busy writing up a paper on the pulsar we just discovered a month ago in the supernova that went off a month ago—rather two years ago. It is about the size of Washington, D.C. It has the mass of the sun. It is spinning at 2000 times a second. The surface is moving at 40 percent of the speed of light, and it has disrupted my entire life.

[Laughter.]

Mr. WALGREN. Let me just say we are probably more interested in the supernova than what will be said by any of the witnesses today, but we have a very informal process and you don't need to feel that you have slighted it in any sense.

Dr. MULLER. In 1980, I wrote an article in Science Magazine describing my perspective on the funding crisis in science. Since that time there has been some notable attempts to improve the situation, but in general my judgment is that the situation has gotten worse.

I am proud of my scientific record, but I am sad to point out that most of my best work was not supported by the National Science Foundation, nor could it have been, given the structure of the Foundation.

As my article in 1980 pointed out, each of the three programs cited in my Waterman Award had been repeatedly turned down by the National Science Foundation. The—in 1980 I naively believed that the Waterman Award would change my own experiences, and I was totally mistaken.

The discovery of the new pulsar last month was made possible by funding from the private Ann and Gordon Getty Foundation. The NSF, by its structure, is designed to support ongoing work in the scientific mainstream and is poorly suited to identify or support truly forefront innovation.

Ironically, it was the Navy, through the ONR, that was once the most effective supporter of physics. Unfortunately, that connection was severed during the Vietnam War by the disastrous Mansfield amendment.

A colleague of mine, Professor Arthur Rosenfeld, recently developed some exciting new technology for energy conservation. He approached the General Electric Corporation for support, and they turned him down. They suggested that he apply for venture capital or make his proposal to a small startup firm. If the inventions succeeded, they said, they would buy up the firm.

That story encapsulates for me both the strong points and the weak points of American technical innovation. Large companies do not like to innovate. That is the bad news. The good news is that venture capital is often available.

The National Science Foundation is like General Electric. Its decisions are made largely by committee or, even worse, by consensus.

In the last month I had two proposals turned down by the NSF. They were both excellent proposals, but the NSF did what it had to do. I expected them to be turned down. This is not cynicism. It is practical experience. It is not sour grapes because I have been able to receive funding for my less innovative projects at the NSF and for my most innovative projects elsewhere. In effect, I have had access to venture capital, some of it from the U.S. Government through those programs that are given block funding, unable to make decisions at the local level.

Two innovations in the NSF have been bold moves in the right direction. I strongly support the Presidential Young Investigator program and the new Science and Technology Centers program. Both of these programs recognize that true scientific innovation comes by finding the best people, regardless of the state in which they reside, and supporting them regardless of how good a proposal they are able to write.

My friends in the venture capital business take the same attitude. They tell me that great ideas, great proposals in science, are a dime a dozen. What they are looking for are great people, people dedicated to their work, people capable of solving problems as they arise. This is the approach taken by the PYI and the Science Centers programs.

I believe that if I had absolute power I could take the budget of the NSF and spend it with ten times the effectiveness and could do it with little effort. But then again so could you if you had absolute power.

Scientists will never agree on what are the best projects, and having them judge proposals only leads to conservative investments. But scientists are very good at agreeing who are the best scientists. With just a few iterations, I would be able to find the top 1000 active scientists in this country. I could do this by taking all of the scientists, getting their lists of the best scientists, and then going to those scientists and asking them for their list of the best and then go to those scientists and ask for their list of the best. My experience is that with iterations we could converge on essentially the same lists.

Then I would give each of these 1000 scientists perhaps a million dollars each to spend on whatever research they wanted to spend it on. The money spent in this way I believe would be far more productive in producing scientific innovation than by doling it out for proposals that achieve the consensus of their peers.

In effect, this is what is done on a more limited scale by the Presidential Young Investigator program, and certainly it is what the Waterman Award does. It gave me $150,000 to spend as I pleased on research, but in retrospect I would say it was worth $1 million to me because it was unrestricted and because of the ability it gave me to do projects that I thought were potentially very exciting.

A grant last year from the Getty Foundation of $50,000 enabled us to discover the new pulsar.

A scientist once said that "basic research is the process of going down dark alleys seeing what is there, hoping to find something." Some people are more observant than others, and they have a better instinct at finding interesting things. They are the ones you want going down the alleys. You don't want to ask them for proposals, which are really attempts to convince other less able scientists which dark alleys are most likely to be productive.

There are lots of details which would become part of a practical system. Decentralization along the lines of the Centers must be continued. More decisions on funding must be made by scientists close to the science, and not by administrators in Washington or even by Congress. Congress must pull back from its own version of micromanagement; that is, from determining priorities within basic science and even within scientific disciplines.

In these hearings you are particularly concerned about big projects, including the space station, the human genome project, and the SSC. I have strong feelings on all of these projects, especially on the space station, but let me instead give you some of my general thoughts about how I believe decisions on large science should be made.

It is absolutely false and misleading to think that large projects such as these build up a basic capability in science more effectively than if the money were to be put into smaller projects. These large projects allow feats of science that otherwise could not be achieved, but they are mostly counterproductive for the training of young scientists. Instead, they build large technological structures with lives of their own, structures that we fear to dismantle because they cannot be rebuilt. The very fact that they cannot easily be rebuilt gives lie to the claim that they train young scientists.

We should build great science projects. They are great achievements. They are thrilling, not just to the scientist but to the lay person. They answer questions that have bothered all people for as long as we have recorded history. They are the adventure of the twentieth century and soon the twenty-first, for which we will be remembered just as we remember the sixteenth century for the exploration of the earth and the eighteenth century for the discovery of human rights.

But these projects by their very nature of conservative projects. They have already been sifted through. They have been sifted through the criticism of a thousand scientists.

Just as President John Kennedy was assured by engineers that the moon was an achievable goal before he announced it as a national one, so are these projects ones that will not fail. They do not represent the forefront of science. They are the leading edge of technology going in a direction that is pretty much predictable, with a few surprises, and more answers than questions.

More importantly, as I mentioned, they do not train people in science. At most, they train people in pushing an advanced technology a little further. They even destroy the careers of many excellent scientists by pushing them into administration, and I know this from several cases of close colleagues of mine who have had their careers essentially destroyed by involvement in two cases with NASA.

A scientist is one who relishes working with the unknown, someone who is willing to live with that uncomfortable feeling of not understanding what is going on around him. He is someone who is willing to spend an enormous amount of time and effort on a project that his instincts tell him may yield a big payoff, even though his colleagues, his peers believe he is wasting his time. A scientist is someone who is good at wasting his time. You don't learn that skill from these big projects.

Big scientific projects are important, exciting, and far more worthy of support than many other projects that will be funded, but they will not keep us at the forefront of science any more than being the world's largest producers of cars kept us at the forefront of automobile technology.

I disagree with several of those who testified this morning as I listened to their criteria for evaluating programs and even wondered for a while what improvements I could make in their clever formulas. But then I thought hard about their criteria and asked myself whether these criteria would have worked in the past to support those great innovations in science that we are now so proud of, and the answer was "no."

I believe there are no formulas. We cannot decide on these great issues by trying to evaluate the effect on the economy of spinoffs or the effect on education of providing jobs or the effects of X on Y. Like struggling hard to enjoy oneself, the method is self-defeating.

Pushing the decision into a formula makes the possibility of a bold decision impossible. Boldness is the casualty of calculation. Science not only thrives on boldness, but it demands it. Without boldness, science perishes.

The unhappy message I have to convey is that I do not believe that Congress can play an important role. There are some things that Congress does well, but play the venture capitalist is not one of them. The President must listen closely to as many great scientists as he can stand to listen to and then guess whether the country will be sufficiently inspired by a large scientific effort that they will be willing to pay the bill.

Of course, you, Congress, are the judge of whether to pay the bill. You may decide that the people are not inspired, inspired to pay for an SSC, a genome map, or a space station. But it is wrong to put these various categories—these various projects in one category called "large science," and then try to see how the money should be divided between them. You will get a project devised by a committee with all of the well-known disadvantages of the proverbial camel.

One role that Congress can play is in the greater support of medium-scale science, of which I have heard no mention this morning. This does train scientists. It is a scale at which I believe true innovation can be done, at least in physics. Congress can endorse the support of productive scientists as opposed to scientific projects, but detailed centralized planning is counterproductive. We should not run our scientific funding programs by emulating the methods that the Soviet Union has used to run their economy.

Thank you.

[The prepared statement of Dr. Muller follows:]

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Testimony of Dr. Richard A. Muller

Department of Physics and Lawrence Berkeley Laboratory
University of California, Berkeley California 94720

to the U.S. House of Representatives
Committee on Science, Space, and Technology
Subcommittee on Science, Research, and Technology

Hearings on the Adequacy, Direction and Priorities for the American Science and Technology Effort

February 28, 1989

BIOGRAPHY

I am a professor of physics at the University of California at Berkeley. My contributions of which I am the most proud is the following:

1. Discovery in 1974 that the Earth and Sun (and all nearby stars) are moving at about 1 million miles per hour through the Universe (called "The New Aether Drift")
2. Invention of "AMS" (Accelerator Mass Spectrometry) in 1978, recently used (by others) to date the Shroud of Turin, and prove it was a medieval fake.
3. the Nemesis theory (1984), still controversial, that explains the death of the dinosaurs as due to the impact of a comet, triggered by another star orbiting the sun ("Nemesis")
4. theory explaining the reversals of the earth's magnetic field (1987)
5. discovery of 7 exploding stars (supernovas) by construction of an automated telescope
6. the discovery on January 18, 1989, of a pulsating star ("pulsar") in the midst of the supernova that was visible in the southern hemisphere in 1987.

In addition, I spend two months every summer working on U.S. national security, and spent three years in discussions with Soviet scientists as part of a National Academy of Sciences panel on arms control. I have been awarded the NSF Waterman Award, the Texas Instruments Founder's Prize, and the MacArthur Foundation Prize Fellowship. I hold two patents on biomedical instrumentation. I am the author of a recent popular book, "Nemesis, the death star."

FUNDING OF SCIENCE

I must apologize in advance for this testimony; I had hoped to have more time to prepare it, but my schedule was thrown into total disarray by our discovery just a month ago of a fascinating, young pulsar in the heart of the Supernova that exploded two years ago. The pulsar has the mass of the sun, the size of Washington D.C., and it is spinning at 2000 times every second. However even though I have not had adequate time to polish my sentences, they are the results of many years of thought. I enclose a copy of an article I wrote in 1980 (*Science*, **209**, 880-883)describing my perspective on the funding crises in science at that time. In the last decade there have been several notable attempts to improve the situation, but in general the situation has gotten worse.

I am proud of my scientific record, but sad to point out that most of my discoveries were not supported by the National Science Foundation, nor could they have been. As my article in 1979 points out, each of the three programs cited in my Waterman Award had been repeatedly turned down by the NSF. The discovery of the new pulsar last month was made possible by funding from the private Ann and Gordon Getty foundation. The NSF, by its structure, is designed to support on-going work in the scientific mainstream, and is poorly *suited to identify* or support truly forefront innovation.

A colleague of mine, Professor Arthur Rosenfeld, recently developed some exciting new technology for energy conservation. He approached the General Electric Corporation for support, and they turned him down. They suggested that he apply for venture capital, or make his proposal to a small "start-up" firm. If the inventions succeeded, they said, they would buy-up the firm!

That story encapsulates for me both the strong points and the weak points of American technical innovation. Large companies do not like to innovate, that is the bad news. The good news is that venture capital is available.

The NSF is like General Electric. Its decisions are largely made by committee, or (even worse) by consensus. In the last month I had two proposals turned down by the NSF. They were both excellent proposals, but the NSF did what it had to do. I expected them to be turned down. This is not cynicism, it is practical experience. It is not sour grapes, because I have been able to receive funding for my less innovative projects at the NSF, and for my most innovative projects elsewhere. In effect, I have had access to venture capital, some of it from the U.S. Government through those programs that are given block funding.

Two innovations in the NSF had been bold moves in the right direction. I strongly support the Presidential Young Investigator program, and the new Centers program. Both of these programs recognize that true scientific innovation comes by finding the best people, regardless of the state in which they reside, and supporting them regardless of how good a proposal they are able to write. My friends in the venture capital business take the same attitude. They tell me that great ideas (great proposals) are a dime a dozen; what they are looking for are great people, people dedicated to their work, people capable of solving problems as they arise. This is the approach taken by the PYI and Centers programs.

If I were King, I could take the budget of the NSF and spend it with 10 times the effectiveness, and I could do this with little effort. (Please forgive the conceit; I realize that there are many problems in politics that would go away if the people given the responsibility also had the power.) My method would be as follows:

Scientists will never agree on what are the best projects, and having them judge proposals only leads to extremely conservative investments. But scientists are very good at agreeing on who are the best scientists. With just a few iterations, I believe I would be able to find the top one thousand active scientists in the country. (Note: not the award winners, but the *active* scientists.) I could do this by asking all the scientists for their list of the best, and then asking these scientists for their list of the best. By the next iteration, I believe that most lists would agree.

Next, I would give each of these 1000 scientists $1 million per year to spend on whatever research they wanted. The money spent this way would, I believe, be far far more productive in producing scientific innovation than by doling it out for proposals that achieved the consensus of their "peers". In effect, this is what (on a more limited scale) the PYI program does. It is certainly what the Waterman Award does. The Waterman Award to me was $150,000, but it was worth nearly $1M to my research because of its unrestricted purpose. And a grant last year from the Getty Foundation of $50,000 enabled my colleagues Carl Pennypacker, John Middleditch and myself (and many others involved in our collaboration) to discover the exciting new pulsar.

I would hope to spend half of the NSF budget every year on supporting the best scientists doing whatever research they thought best. A scientist once said that "basic research is the process of going down dark alleys seeing what is there, hoping to find something." Some people are more observant than others, and they have a better instinct for finding interesting things; they are the ones you want going down the alleys. You don't want to ask them for proposals, which are in reality attempts to convince other, less able scientists which dark alleys are most likely to be productive.

What about the young scientist, or unlucky scientist, or the one with nothing but a good idea? Would I leave them unfunded? No, I would leave half of the NSF budget for proposals, and the peer review system. That is still a great deal of money.

There are lots of details which would become part of a practical system. Decentralization, along the lines of the Centers, must be continued. More decisions on funding must be made by scientists close to the science, and not by administrators in Washington, or even by Congress. Congress must pull back from its own version of micromanagement, i.e. from determining priorities within basic science and even within scientific disciplines. There are, of course, certain areas of science and technology that are known to apply directly to the national interest, and work in these field should be funded even if it is not being done by the "best" scientists. These areas should not, however, compete with basic science, any more than (for example) one should mix the corn you intend to sell with the corn you will use for seed next year. Basic science is a long-term investment, and it will not look attractive to many people when compared with short-term investments.

My proposal may not be politically possible. It may happen, eventually, anyway. Democracy was not politically possible in the 17th century. I give these ideas in part as encouragement to those of you who are moving scientific funding in the right direction, and need moral support..

FUNDING OF LARGE SCIENTIFIC PROJECTS

In these hearings you are particularly concerned about "big projects" including the space station, the human genome project, and the SSC. I have strong feelings on all of these projects, but I don't believe that is what you want from me. Let me instead give you some of my general thoughts on how decisions on large science should be made.

It is absolutely false and misleading to think that large projects such as these build up a basic capability in science more effectively than if the same money were to be put into smaller projects. These projects allow feats of science that otherwise could not be achieved, but they are mostly counterproductive for the training of young scientists. Instead they build large technological structures with lives of their own, structures that we fear to dismantle because they cannot be rebuilt. The very fact that they cannot easily be rebuilt gives lie to the claim that they train young scientists.

We should build great science projects. They are great achievements; they are thrilling, not just to the scientist, but to the lay person. They answer questions that have bothered all people for as long as we have recorded history. They are the adventure of the 20th century (and soon the 21st) for which we will be remembered, just as we remember the 16th century for the exploration of the earth, and the 18th century for the discovery of human rights. But these projects by their very nature are conservative projects. They have already been sifted through the criticisms of a thousand scientists. Just as President John Kennedy was assured by engineers that the moon was an achievable goal before he announced it as a national one, so are these projects ones that will not fail. They do not represent the forefront of science; they are the leading edge of technology going in a direction that is pretty much predictable, with a few surprises, and more answers than questions. Most importantly, they do not train people in science. At most, they train people in pushing an advanced technology a little further. They even destroy the careers of many excellent scientists by pushing them into administration. A scientist is someone who relishes working with the unknown, someone who is willing to live with that uncomfortable feeling of not understanding what is going on around him; he is someone who is willing to spend an enormous amount of time and effort on a project that his instincts tell him may yield a big payoff, even though his colleagues ("peers") believe he is wasting his time. A scientist is someone who is good at "wasting his time." You don't learn that skill from these big projects.

Let me reiterate, for clarity. Big scientific projects are important, exciting, and far more worthy of support than many other projects that will be funded. But they will not keep us at the forefront of science any more than being the world's largest producer of cars kept us at the forefront of automobile technology.

I will disagree with many of those who testify before you. I read of their criteria for evaluating projects, and I even wondered for a while what improvements I could make in their clever formulas. Then I thought hard about their criteria, and asked myself whether these criteria would have worked in the past, to support those great innovations in science that we are now so proud of. The answer was no. There are no formulas. We cannot decide on these great issues by trying to evaluate the effect on the economy of spinoffs, or the effect on education of providing jobs, or the effects of X on Y. Like struggling hard to enjoy oneself, the method is self defeating. Pushing the decision into a formula makes the possibility of a bold decision impossible. Boldness is the casualty of calculation. Science not only thrives on boldness, but it demands it. Without boldness, science perishes.

The unhappy message I have to convey is that I do not believe that Congress can play an important role. There are some things that Congress does well, but play the venture capitalist is not one of them. The President must listen closely to as many great scientists as he can stand to listen to, and then guess whether the country will be sufficiently inspired by a large scientific effort that they will be willing to pay the bill. Of course you are the judge of whether to pay the bill. You may decided that the people are not inspired, inspired to pay for an SSC, a genome map, or a space station. But it is wrong to put these various projects in one category, "large science", and then try to how the money should be divided between them. You will get a project devised by a committee, with all the well-known disadvantages of the proverbial camel.

I have strived in this testimony to avoid political realities; they are not my area of expertise. Many of my fellow scientists have worked to become experts at playing the politics of science, and they may dismiss my thoughts as hopelessly naive. However I believe that even with them my thoughts will strike a deep chord of truth, and I hope may even provide some guidance through the maze of practical problems.

Mr. **Walgren**. Thank you, Dr. Muller. I appreciate that.

Dr. Pollard.

STATEMENT OF DR. THOMAS POLLARD, PROFESSOR AND DIRECTOR, DEPARTMENT OF CELL BIOLOGY AND ANATOMY, JOHNS HOPKINS UNIVERSITY MEDICAL SCHOOL, BALTIMORE, MARYLAND 21205

Dr. **Pollard**. I am going to take a different tack. I am used to working in a lab with students and post-docs, but I want to be here to make a strong plea for Congressional leadership in establishing the priority for education and science in this country.

I am happy to respond to your specific questions, but I am particularly concerned that the very nature of the questions that we are discussing here today have missed the main point.

It seems to me the main point is that no matter how many dollars are available for the elite to do research in research universities, that the country is going to go to hell if our educational system is rotten at the core.

And I have heard expressed here today, both among the witnesses and among yourselves, the widespread realization that our elementary and secondary educational system really has become mediocre at the best.

So what in the heck are we going to do about this? Because if we don't do something we are going to lose our leadership not only in science and technology, but economically and militarily as well.

One of the things we are not going to do about it is to invest $250 million, like President Bush suggested in the State of the Union message recently. There are about 50 million K through 12 students in this country, and so his investment of $250 million would be $5 per student, an increment of .1 percent in the total expenditures for education in this country.

Now, a more realistic figure might be 100 times that figure, that big. Something on the order of $23 billion a year might have a small amount of impact on improving our basic educational system, which is vital for us to do.

Now, even this would only be a 13 percent increase above current levels. Now, these funds will obviously have to come from investments at many levels, local, state and national levels, but it seems to me that Congress is the only body in this country that is influential enough to initiate such an investment.

An added investment of $23 billion per year is a huge amount of money, but it is only 25 cents per person per day. Citizens are likely to be receptive to Congressional leadership. In Maryland, my state, 80 percent say that they would pay higher taxes to support the schools.

The federal science agencies, such as NSF, do not have these resources, obviously, but they can contribute by providing advice and funding research on how to best invest this money.

Your committee is to be congratulated and commended for its leadership role in supporting the Science Education Directorate at the NSF, who might very well be able to contribute to this.

Now, the rest of your—that is my main point. The balance of your first question had to do with the adequacy of support for small-scale scientific research, university infrastructure, and science education—and the situation there is highly variable. It is all across the map.

In biomedical research, where I do my work, there actually is quite a bit of money. It is hard to get, and there are very many good ideas that are going unfunded because of the lack of total resources. On the other hand, I don't really believe, to be honest with you, that steady progress is being prevented by this limitation of resources in our field.

On the other hand, there are other areas of science where the limitation of resources is having a tremendously negative impact, and a couple of examples that I can cite are the earth sciences in general and the training of biomedical scientists in particular.

It is shocking to learn that this year the funds are so short in parts of the NIH that major federally supported predoctoral programs at Duke, Cal Tech, and the University of Pennsylvania will have to be closed down because they have run out of money. This is ridiculous.

Now, your second concern had to do with big science versus little science, and here the situation is also highly variable.

In some areas, such as the earth sciences, there is a trend toward big science, and in this case I have to say it is sort of moderate sized sciences. The whole budget for earth science is less than $100 million. Now, the trend toward these big projects has been actually misguided, according to my colleagues in that field. What has happened is a lot of big projects encouraged by funding agencies have replaced a whole lot of little projects. Some of the big projects work, but some of them fall flat on their face and certainly were not the investment that could have been made in a lot of smaller projects that might have been more innovative.

Now, biologists as a group strongly favor small projects. Number one, because they promote innovation and, number two, because in contrast to some of the previous witnesses, the future in biology is extremely hard to predict. I think getting a bunch of soothsayers together to predict what is going to happen in the future in any area of science, particularly in biology, is not time and money well spent.

I think we would all favor small initiatives. Although the cooperation and collaboration afforded by the human genome project would be beneficial, that it would trickle down to a lot of small projects.

If you want, I will comment on the AIDS thing, but I don't think I have time to say something about that right now.

The third concern was on the quality and numbers of students entering science. My colleagues have already commented on the desperate situation in many fields where there has been a strong decline in the number of Americans participating in advanced training in graduate programs and they have been supplanted largely by foreign graduate students. And although this is a wonderful contribution to world culture, it certainly raises questions about whether we are going to be competitive economically in the future if we aren't training technically qualified people at the graduate level.

In biology the top programs are actually full of students and we are doing reasonably well. But the funding of these students is very tricky these days because NSF has very little money to support students and the NIH has transferred a lot of the training responsibility over to research grants, which is a very inflexible way and not a very innovative way to promote graduate education.

There is a serious crisis developing in the medical field because the number of applicants to medical schools has dropped in half during this decade. And it is particularly severe in the area of academic medicine, where fewer and fewer medical students are interested in pursuing careers as investigators.

The problem here is that these are the very people that are going to be needed in the next century to apply the fruits of the molecular biology revolution to the clinic. They are being driven out of academic medicine by the very long period of time that some subspecialty boards are requiring for their training and due to a great deal of uncertainty about whether research funds will be available to them once they have made this commitment in the future.

Now, your final question concerned the ability of the scientific community to establish priorities. And you have certainly got a lot of mixed messages on that here this morning. And I think that is because most bench scientists think this is a totally hopeless proposition, particularly when it comes to making value judgments about the relative merits of physics versus biology or military research versus biomedical research.

On the other hand, I am much more sanguine about scientists helping with such decisions and[3] leaving these difficult matters in the hands of those who lobby for one cause or another, largely for the economic benefit of themselves or their constituents.

Scientific policies must be established solely on the basis of scientific merit and national needs. And the technical issues are so complex—and I think you admitted that this morning—that only well-informed scientists are in a position to make such judgments. Lobbyists and politicians probably shouldn't be making judgments about scientific merit.

Now, I believe that the scientific community can help you with this. And evidence for that includes the very great success, at least at the NIH, of peer review study sections where there is virtually always unanimity about the value of various research proposals, which I should point out in that setting often go to the people who are considered to be the most innovative scientists rather than the ones who are just following some sort of a routine line of research, as suggested by Dr. Muller.

On the other hand—and some evidence of that is that—I am a member of a panel that wrote a report on research opportunities in basic biology for the NRC. You will have that later this spring or early this summer.

Although there is a heck of a lot of different people doing biology, we could achieve a consensus about what was important and what should be done in the future. The discouraging thing is that even though this panel could agree on what should be done, it is unlikely that these views will ever be implemented.

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[3] Dr. Pollard, after reviewing the transcript, indicated that "and" should be changed to "than" to reflect his intended meaning.

The problem is the same one that keeps us as a country from investing in sufficient resources in education, scientific research, and even industrial modernization. The present day Americans have a terrible time postponing immediate gratification to make the long-term investments required to remain competitive in many areas of life. We live in a short-sighted, get rich quick, credit card culture. This is not the best way to advance science and technology, of course.

For example, most leading biologists would support my view that now is the time to invest in basic biology at the expense of more applied work, and I can give you specific examples of that later.

On the other hand, a large number of influential special interest groups need to be heard from. Due to their immediate concerns about health, they will urge funding agencies to invest now in applied medical research on some disease which, in my opinion, will frequently be premature.

To return to my main theme, I urge you in Congress to respond to the great need that this country has for leadership and turning our attention to the value of long-term investments in the education of our citizens and the acquisition of basic knowledge about nature.

Given adequate support and some patience, science can deliver the knowledge that we need to deal rationally with many national problems, including most diseases, the energy supply, and the environment.

The situation in education is desperate and few people have the vision necessary to make the sacrifices now that are necessary to assure the future of our country.

Now, in this leadership vacuum, Congress has an opportunity to set the agenda by investing heavily in education and science and by insisting that both the Congress and the President have highly visible, strong advice about science and education. In my opinion, the President isn't going to take care of this. The ball is in your court, and we are counting on you.

[The prepared statement of Dr. Pollard follows:]

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Statement of Thomas D. Pollard, M.D. before the Subcommittee on Science, Space and Technology of the U.S. House of Representatives. Hearings on The Adequacy, Direction and Priorities for the American Science and Technology Effort. February 28, 1989.

Thank you for this opportunity to share my views on this important subject. I am Thomas D. Pollard, Professor and Director of the Department of Cell Biology and Anatomy at the Johns Hopkins Medical School. I am here representing only myself, but I note that I am past president of the American Society for Cell Biology, a member of the Executive Committee of the Biophysical Society and a member of the panel at the National Research Council that wrote the report on Research Opportunities in Biology, due later this spring.

The major focus of this hearing is the adequacy and distribution of funds for science and technology. I have been asked to comment on approaches to establish research priorities and the merits of big versus little science from the perspective of a university-based investigator in the biomedical sciences. I have views on the specific questions that you have raised, but I think that the very nature of the questions misses the main point. That is, no matter how many dollars are available for the elite to do their research at the university level, the county will fail to compete economically and militarily if our basic educational system is rotten. None of us want to see the country relinquish its world leadership in science and technology, and I urge you to look first at the deplorable condition of our educational infrastructure at the primary and secondary levels.

The situation is urgent, because our generation is currently presiding over the decay of our educational system, which due to neglect has declined from a position of leadership 40 years ago to a level that only charitably can be described as mediocre. I am sure that each of you has been as shocked as I to read the results of recent surveys of science literacy in our population and to learn that our primary and secondary students are no better than second or third rate compared with the youngsters from our major economic competitors.

I want to impress you members of Congress that you need to exert strong leadership to convince your colleagues in government and the citizens of this country that we as a people need to make an immediate, large investment in education, science and technology at levels that politicians have never dared to propose. To be blunt about it, you have to escape from the shortsighted conventional wisdom that we need to hold the line on taxes in order to be happy in this country. Instead, Congress has an obligation and opportunity to provide the leadership to help the country recognize the only way to assure the long term security and economic success of this country is to make modest sacrifices now to make possible an adequate investment in the future.

The amount of money needed to reverse the progressive decay of our educational system, especially at the primary and secondary levels, is probably not known, but it is certainly much more than the $250M suggested by President Bush in his recent State of the Union message. As you know, there are 46,000,000 primary and secondary students in the US, so $250M amounts to only $5 per pupil or an increment of only 0.1% in total expenditures for primary and secondary education. An increment of $500 per pupil, a total of $23 billion per year is a more realistic figure. Even this is only a 13% increase above the current inadequate level. The federal science agencies such as NSF do not have these resources but they can contribute by providing advice and funding research on how to spend new money. These funds would obviously have to come from combined investments at the local, state and national levels, but it seems likely that only Congress is influential enough to initiate such an investment. An added investment of $23 billion is a huge amount of money, but it is only 25 cents per person per day. Is this investment too much to reverse our economic decline?

The balance of your first question concerned the adequacy of support for small-scale scientific research, university infrastructure and science education. The adequacy of support for investigator-initiated research and university infrastructure is highly variable:

(a) - In many areas of basic biomedical research, funds are hard to get, more could be done with greater resources. On the other hand the struggle to obtain funds is probably not preventing steady progress. One consequence of tight funds is that major research centers, where much of the leading research is done, are gaining an increasing fraction of the total funds. Without doubt this is more cost effective than spreading funds widely, but the financial health of a large number of smaller research centers is being compromised. The second rank of investigators, who often make valuable contributions to the general research effort, are being cut off because only a small fraction of approved grants are being funded by either the NSF or the NIH.

(b) - In some areas of biomedical research that have passed their prime, there are probably more than adequate funds to carry on the research effort.

(c) - Other areas of science are pitifully underfunded. Two examples are the earth sciences in general and biomedical research training where the situation has reached crisis proportions this year at the NIH. Due to an adjustment in student stipends in the face of a fixed appropriation, the institute with the largest responsibility for research training at NIH will be able to fund this year only 20 postdoctoral fellows for the entire country and will have to close down major institutional training programs at such leading institutions as CalTech, Duke and the University of Pennsylvania. There are many other poorly funded fields including areas of biology dealing with natural history - physical anthropology, systematics and so on. With the massive losses of natural areas in equatorial regions, research in these areas is more valuable than ever before.

Your second concern had to do with big science vs. little science. Here the situation is again highly variable. In the case of the Human Genome Project, the coordination and cooperation afforded by this big science approach is clearly beneficial. In other areas such as the earth science, the trend toward big science seems to have been misguided. There big projects are being proposed because they are encouraged by funding agencies. Some are successful, but some not - they are merely bigger and not as good as an investment as the large number of small projects that they have replaced. On the balance, most biologists strongly favor small, investigator-initiated research projects. This is justified by the unpredictability of biological research. A large number of small projects always seems to produce more innovative research. I should note, in case that it has escaped your attention, that some big initiatives such as the AIDS initiative are having a negative impact on the rest of biology. Despite what one reads in the papers, there are essentially no new funds available for AIDS research at the NIH, so most of the dollars have been diverted from other programs. One way this has been paid for is to make arbitrary administrative cuts called "downward negotiations" in all other grants - currently at the level of about 10% per year. Since these awards have already been cut down by peer review committees to the minimum funds required to do the proposed work, most of us are scrambling to keep our labs functioning. I also need to point out that the sort of unfocused basic research that is now being cut to support the AIDS effort is precisely the type of research that provided the basic knowledge and research infrastructure that resulted in the rapid identification of HIV as the causative agent in this disease. Consequently, the current sacrifices in fundamental research may come back to haunt us in the future.

Your third concern was the quality and numbers of students entering science. The top graduate programs in biology are full of students but the competition for excellent PhD students intensifies every year. In many areas of science and engineering, foreign students are on the verge of outnumbering americans. This is certainly a valuable contribution to world culture and economic progress, but the decline in participation of american students in graduate education is alarming and raises concern about our ability to compete with other nations in future decades. In the biomedical sciences there is widespread concern that a shortfall in the number of PhDs will result in a shortage of candidates to fill the large number of faculty positions becoming available in the late 1990's. Finding funds to support these graduate students in biology has gotten very complicated, because funding for training programs, at least at NIH, has remained flat for more than a decade. A large proportion of the training effort has been transferred to research grants, a very inflexible training mechanism. Even more striking than the problems in the PhD area, the number of applicants to medical school has dropped in half during the 1980's. Although there is likely to be an adequate supply of practitioners, the shortage of physicians prepared for academic careers is reaching crisis proportions. The shrinking number of MD investigators is alarming, because they will be needed to apply the fruits of the molecular biology revolution in the clinic. Medical students are now discouraged from careers as investigators by the long period of training required by many subspecialty boards and the real fear that neither research funds nor adequate reimbursement for part time clinic service will be available in the future. Some of these same fears contribute to the withdrawal of many talented students from careers in other fields of science.

Your final question concerned the ability of the science community to establish priorities. Many scientists think that this is a hopeless proposition, particularly when it comes to making value judgements about the relative merits of physics vs. biology or the benefits of biomedical research vs military research. On the other hand, I am much more sanguine about scientists helping with such decisions than leaving these difficult matters in the hands of those who lobby for one cause or another largely for the economic benefit of themselves or their constituents. Scientific priorities must be established solely on the basis of scientific merit and national needs and only well-formed scientists are in a position to make such judgements.

I can provide an example from biology about how such a consensus based on scientific merit can be reached. When the NRC Panel on Basic Biology started its work, there were representatives from most fields of biology - each with a traditional approach. You might imagine than there would have been a great divergence of opinion on future research opportunities. There was not, because biology now has common language and approach focused on understanding life processes at the cellular and molecular levels as well as a consensus on the value of various approaches among the broad range of research opportunities. Nonetheless, even though these leaders could agree on broad priorities, their views are unlikely to be implemented because of political pressures in the current system. The problem is the same problem that keeps us from investing sufficient resources in education (and all of science for that matter) and has diminished the competitive stature of American industry in the international arena. Present day Americans have a terrible time postponing immediate gratification to make the long term investments required to assure the continued leadership of this country in the world. For example, the most leading biologists would support my belief that now is the time to invest in basic biology at the expense of more applied work. On the other hand, a large number of influential special interest groups needs to be heard from. Many of them will urge funding agencies to invest now in applied research on some disease, much of which, in my opinion, will be premature. Due to their immediate concerns about health they are searching for short term solutions rather than being willing to make a long term investment in the basic knowledge.

To return to my main theme, I urge you in Congress to respond to the great need that the country has for leadership in turning our attention to the value of long term investments in the education of our citizens and the acquisition of basic knowledge about nature. Given adequate support and some patience science can deliver the knowledge that we need to deal rationally with problems that science can eventually solve including most diseases, the energy supply and environmental problems. The situation in education borders on the desperate and very few people have the vision necessary to make the sacrifices necessary for our future success as a country. You can lead the way by insisting that both the Congress and the President have highly visible, strong, advice about science and education.

Mr. **Walgren**. And Sherry just left.

I am reminded of a hockey game between Boston College and another team. Boston College was losing 1 to 0, and there were some fans for the other team that got down behind the Boston College goalie and said very gently and quietly, but repeatedly they kept saying—his name happened to be Rod—they said, it is all your fault, Rod. They said that over and over again. It is all your fault, Rod. If it weren't for you, the score would be 0 to 0.

So I guess I kind of feel a little bit that way when you say you are counting on the Congress. It is not—that statement is not misplaced.

You had a follow-on comment?

Dr. **Pollard**. It is not your fault, but you are the ones who are going to have to do something about it.

Mr. **Walgren**. What is your sense as to the focus of efforts to get at education?

We have always sort of supported the NSF's derivative approach to science education whereby supporting the graduate students somehow or another the whole education enterprise was carried out, or an educational enterprise was carried out.

But you are arguing for going back to the base and really trying to put the wood to the broad initial education in science and math.

Dr. **Pollard**. The situation is extremely complicated, as I am sure you understand better than I, but it really boils down to whether or not the country is willing to make the investment or not. The investment at the present time is not very high, and it is not very evenly distributed.

A kid growing up in a poor state or a poor jurisdiction in a wealthy state just doesn't have access to the same sort of opportunity that somebody has who comes from a wealthy home who can go to a private school.

We are wasting our intellectual resources in this country, and the disparity can be tremendous. In the State of Maryland, the difference in per pupil expenditures per year between the lowest and the highest is $2000 per student per year. In one classroom that is a difference in expenditure of $60,000 per year.

Can you imagine what a difference $60,000 would make to those poor students?

I wouldn't want to tell you exactly how to spend it. But that is something the NSF can help you with. And they have got lots of good ideas and programs that you could use in terms of how to spend the money. But somebody has got to be willing to exert the leadership in this country, to say that we are going to go down the drain if we don't put some money into this, and if the taxpayers in Maryland are willing to pay some money, I expect some other people would be as well, but somebody has got to stand up and make that point. The President is not making that point.

Mr. **Walgren**. Dr. Muller?

Dr. **Muller**. I think the problem of science education is one that is much, much deeper than the public score problem. It is a problem that children's parent don't understand science or technology, that the teachers are not there.

Congress admits, as you did this morning, it is largely scientifically illiterate. We just went through an Administration that had no high level scientific advice. In fact, it was the most technically deficient Administration that I know of in my own lifetime. We don't have scientific knowledge even at this level right here, and something has to be done about that.

Having us travel to give a few minutes of testimony helps, but why aren't scientists attracted to Congress?

You need to be talking to scientists all the time. I assume you are in this committee, but there are so many technical committees, technical questions that are being decided all the time without the benefit of good scientific advice.

I am not sure what the problem is, but the problem is not just one that you can just approach by trying to feed it from the bottom. Somehow the entire country has to become more knowledgeable about science.

I am appalled when I listen to people in the Administration or in Congress making statements on television about scientific matters in which they are all wrong. Newspapers are—to be a science reporter is often the lowest prestige position, and it is reflected. There may be one or two newspaper in the country that has decent science coverage.

It is a problem that pervades our society, and hoping to solve this by giving elementary school children better education, that is certainly needed but it will not be sufficient.

Mr. WALGREN. You had mentioned, Dr. Pollard, some thoughts about AIDS, and I would like to give you an opportunity to do that, or touch on that, along with the question of thrust toward applied, as I heard you, versus basis.

Dr. POLLARD. I would be happy to do that.

The AIDS initiative is absolutely essentially, of course, because this is a very serious public health problem.

On the other hand, the funds that are being used for the AIDS program are to a large extent coming from rebudgeting from other programs at NIH, so that a lot of other programs, such as the ones that I turned to for support, are being compromised by this national need. And so it would, of course, have been better to provide additional funds for the AIDS effort and not therefore to have to compromise the other sorts of fundamental research.

An important point here is that the fundamental basic research that is being hurt right now is the very type of research that provided the background which enabled investigators to identify HIV as the causative agent in this disease and to make some progress. So that by compromising support to sort of the background, the fundamental work, an effort like the AIDS effort is likely to come back to haunt us in the future because we will not have that infrastructure to deal with the next problem that comes along.

If I can be personal about the issue about applied versus basic research, I would like to share with you the fact that a year ago I was in the hospital with colon cancer, in very serious shape. And I fortunately appear to have been treated successfully for this. And this very shocking development caused me to do a lot of reading about what was going on in the cancer field. And I was alarmed to read and learn that most of the applied work in the cancer therapy field, particularly in chemotherapy, has not led to any advancements in our ability to treat people.

Now, in contrast, as a cell biologist I am thrilled with the tremendous progress that is being made in understanding the basic mechanisms of cell replication, and so on. The progress in that and many other fields in cell biology is moving along at such a good rate that I think it is safe to predict that some sort of a grand synthesis of information is likely to fall out in the next 10 or 20 years. And when that information is available, then we will be able to do something rational for the first time about patients who have cancer as opposed to putting a lot of money into applied efforts to—for example, in chemotherapy, which are likely to only give us a tiny incremental improvement in the situation.

Most chemotherapy trials are judged successful if they can get a 5 or 10 percent increase in survival, or not even survival, just a disease free interval after the tumor has been discovered. And I think what we should shoot for instead is a long-term solution, which is going to have to be based on advances in fundamental cell and molecular biology and to back pedal a little bit on some of these more applied things.

Now, the trouble is that many people who are supporting the cancer research effort, research efforts on most other diseases, are looking in the short run. They want to do something about their friends and relatives or perhaps themselves, who are ill with one of these diseases. But here I am—I am a patient—and I am telling you that we ought to put our money into the long-range goal of understanding how the system works. And then we can do something that would be more permanent.

There are many other examples I could give you. Politically, it is hard to pull that off. I can get a consensus among most biologists and scientists, including clinicians on this point.

Mr. **Walgren**. The emotions that drive us really drive us in the other way, to want to do something, and that is why I am sure the doctors are doing the chemo and we want a pill, we want to do something to feel as though we are physically providing a remedy.

Dr. **Pollard**. This is where Dr. Muller's point is important. The general level of the understanding of the technical aspects of a question like the treatment of cancer is so low in the population that it is very difficult for those who are in the know to make a point, to score any points anywhere.

So what is happening is perfectly understandable, given our low level of science literacy and understanding, because we are looking for a quick fix. And what I am saying is that there is a consensus out there among the people who are doing the fundamental research that we can provide the answer. And you will have to give us another 10 years to do it and plenty of money, but at that point we could cash in on all of this good research that has been done.

I would suggest that it would be better to sort of back off a little bit on some of the more applied sorts of research in that area. And I am sure other people could think of other examples. I picked that one because of its personal concern to me.

Mr. **Walgren**. Certainly, there is pretty much of an absolute case to be made for providing certain resources to the individual, small, fundamental researcher, which I gather doesn't get us into requiring these big amounts of institutional support, anyway?

Dr. POLLARD. I will shut up in a minute, but let me respond to that.

In biology, to run a laboratory these days costs about—in cell and molecular biology costs on the order of 1-to $300,000 per year exclusive of the professor's salary. So it is not inexpensive. It is much more expensive than math research, for example. But that would be classified, I am sure, as little science initiated by a specific investigator and funding directly to that investigator.

Mr. WALGREN. Dr. Muller?

Dr. MULLER. Let me mention again, in physics what I feel is most lacking is the medium-scale science, particularly the ability for a small-scale project to grow and quickly follow the lead. There are quite a few extremely large-scale projects, and then there are the individual university investigators.

What I believe we need more of are the small size scientific groups of a few scientists working together, particularly having some continuity of funding. Sometimes this can be done at the university if the university is large enough and you can have several faculty members collaborating. But what you really need is to have a—the possibility of having long-term commitments to research scientists who, together with faculty, can work together on medium-scale projects with some hope of expansion of the project if it is successful. And of course along with that they have to recognize that if it is not successful it will be—the money will be later taken away.

But as with many other problems in the government, the continuity of funding is absolutely essential. You cannot just hire somebody for one or two years, and when you are given a one-year NSF contract or two-year NSF contract, it is simply impossible to form these kinds of groups.

The Science Center program I consider a major breakthrough, and I have not seen the scientific support among my fellow scientists of that program which I think it really deserves. I think this is in part because of the perception among many of them that the program was more applied than they had hoped it would be and also because many of them have learned to play the political game and oppose things even at the base that are good for short term possible gain.

But the Science program, the Science Center program, I think is really moving in the right direction, and if that could be expanded, I think it would be a great payoff to science.

Mr. WALGREN. It is in physics where all of these big projects seem to come from. The physicists seem to—high energy physicists seem to be—

Dr. MULLER. High energy physics has a very special history. It has inherited the gratitude of World War II from the public, and for that historical reason it has very large funding.

Ironically, I think the medium-scale projects have their history in high energy physics. Those are tending now to be turned into large-scale projects, which I think is much less effective for scientific training.

When I got my PhD, I can name you 10 graduate students who got their PhD the same year as me working in diverse areas of science. Now, one of them is designing computer architecture. Another is working for an oil company. Another is a chief analyst for the CIA, and I can go on like this. These are all my fellow students.

It is a wonderful training, and I believe that if you can give these students this very basic training in physics they then go out and are extremely productive. If you ask what is the value of the science they are doing, there is none, and then they don't go out and perform these diverse roles in which I think they are really truly highly productive.

I believe the best connection between basic science and the benefit of the society is not the result of that science, but of the discipline that it imposes on the students, where there is such a thing as truth. Many areas of knowledge, there is no such thing as truth. There is only opinion. In basic science we have truth, and that imposes a discipline which is highly beneficial when these people go into other areas of science.

When the graduate students come and they go into these other areas, they give us the true benefit from basic research, the true society—societal benefit.

Mr. **Walgren**. Dr. Fefferman.

Dr. **Fefferman**. That benefit exists, but I wouldn't call it the main benefit from basic science. It seems to me that basic science over and over again has led to very important technological and economic consequences. The lead times can be long, but the benefits are very real and very large.

Mr. **Walgren**. But the discipline really seems to be central to what we hope for people, anyway, and I get so struck by the passivity of television. I was at Disney World and went to Epcott Center, and it was a terribly passive experience, even that place, which is supposed to be—well, maybe there are parts of it that aren't, but for the masses, anyway, for the mass of visitors, how passive it was. And you wonder whether there isn't something in our system where we can get away from being sort of lulled along. And I don't know what it could be. Perhaps it could be a real vibrant educational program. But doggone it—and maybe more so in science, where there is an acting going on, a doing.

Dr. **Muller**. I hope you have an opportunity to visit the Exploratorium in San Francisco sometime, which I consider the best scientific experience for any young, middle aged, or old person that you can have. It is extremely active, with hundreds of exhibits, each one of which is hands-on or you do something.

Mr. **Walgren**. Yes.

Dr. **Muller**. Such centers as the Exploratorium are beginning to grow around the country, and it is one way of passing on the thrill and understanding of science if they could receive more support.

Mr. **Walgren**. Dr. Fefferman, you indicated that—was it the David Report—called for 2600 individual support elements?

Dr. **Fefferman**. Yes.

Mr. **Walgren**. Where are we now? At what levels are we now?

Dr. **Fefferman**. In 1984, when the report was issued, I think we were at 1300, and now we have progressed to 1490-something. For our little science this is a big problem. This is something that has to be taken care of.

Mr. **Walgren**. And are there people there that could use the additional numbers well?

Dr. **Fefferman**. Oh, absolutely. Dr. Muller has indicated how he didn't get NSF funding for his most interesting projects and, regrettably, math is full of examples of such things. The NSF I think has done the best job that one could do, given the very low level of funding for individual grants. But every mathematician has his favorite horror story of somebody who lost an NSF grant and then the next year solved an important problem or somebody who solved an important problem and then the next year lost his NSF grant. It happens all the time.

Mr. **Walgren**. I see.

Well, gee, I really appreciate all of you coming, and we will be looking for the long run, Dr. Pollard, in lots of ways. So we wish you the very best in that particular instance and your science as well.

I hope we can take some of the energy that each of you in your individual ways obviously have, and maybe the Congress can make a good contribution in this area and help things go in the right direction.

So on behalf of the committee, thank you all for going out of your way to be here. We appreciate it.

[Whereupon, at 12:45 p.m., the subcommittee was adjourned, subject to the call of the Chair.]

THE ADEQUACY, DIRECTION AND PRIORITIES FOR THE AMERICAN SCIENCE AND TECHNOLOGY EFFORT

WEDNESDAY, March 1, 1989

House of Representatives,
Committee on Science, Space, and Technology,
Subcommittee on Science, Research and Technology,
Washington, D.C.

The subcommittee met, pursuant to call, at 9:35 a.m., in room 2318, Rayburn House Office Building, Hon. Doug Walgren [chairman of the subcommittee] presiding.

Mr. Mineta [presiding]. The Subcommittee on Science, Research, and Technology will please come to order.

This morning, we will continue the discussions on the adequacy and allocation of Federal support for science and technology. Our very fine chair is on his way in right now, and I thought, in the interests of time, that I would go ahead and start the subcommittee hearings.

It does give me a great deal of pleasure to have this opportunity to continue these hearings, because it is appropriate that Mr. Walgren has chosen the whole issue of adequacy, direction, and priorities of our science and technology efforts as the subject of these first hearings in the new Congress. The subject is a great and vital one for all of us.

How we spend our research and development funds directly impacts upon our very health and overall quality of life, and by making careful decisions and spending properly we can further the goals of both our national defense and the protection of our environment. Our decisions in this committee influence the course of research and development both here in the United States and internationally. Moreover, what we decide here can have significant influence on our long-term competitive abilities in the international marketplace. So, in short, we can, by our deliberations, help fulfill the aspirations of our great Nation.

We in Congress often have to choose between equally desirable research activities. In these fiscally restrained times, it is all the more important that we deliberately choose those activities most likely to bring the most important advances, and so the advice and the comments that we will hear today should prove very useful and helpful to us in our deliberations.

President Bush has indicated his high regard for science and technology issues and advice by stating that he will elevate the position of science advisor to the President to the position of assistant to the President, and this should increase the access and power of the Nation's first scientist and give more weight to science issues and science budgets. This is a very positive step, and I think all of us should join him in encouraging him in that endeavor.

I am also interested in the methodologies of priority setting, particularly as it might be accomplished by this or other committees of the Congress that have responsibilities for science issues and agencies, and so I would like to ask, in the absence of the chair, to see whether or not it would be possible for our committee staff to distill the rationale and the methods and the insight that will be presented during these hearings as well as testimony that is going to be submitted in writing into a two- or three-page document for distribution to interested members of the committee and other Members of the Congress. I hope that we would be able to do that with the use of our staff here.

So, again, I would like to commend Mr. Walgren and our able committee—subcommittee staff for organizing a very rigorous schedule of hearings for us in the next few months.

Ms. Schneider, I would be more than happy to yield to you at this point.

Ms. SCHNEIDER. Thank you.

My colleague, Mr. Mineta, I certainly commend for your suggestion that we set our priorities and make it clear to the members of this committee exactly what has been delineated as being first and foremost in the minds of our science and technology policy.

I will say that this hearing is very timely, but let me also add that we have done some research, as have many others, as to the apparent priorities of the science and technology effort in the United States, and I believe that achieving a better balance between civilian and military R&D is fundamental to our national interest and our position in the world economy.

There has been a recent trend that is clearly documented in Federal R&D that I find very disturbing, and that is that the military has received 65 percent of the proposed Federal R&D budget for fiscal year 1990. The result would be that nearly two-thirds of our Nation's effort to develop new ideas, new products, and technologies would be devoted to the military instead of to health, to energy, to the environment, to technologies that will put us foremost in the global competitive market.

Recent increases in the military R&D have upset the historical balance in funding, because from 1965 to 1980 money for R&D was almost equally divided between civilian and military purposes, but since then over 83 percent of the increase in R&D funding has gone to the military.

Inadequate civilian R&D threatens our economic competitiveness. An emphasis on military projects diverts scientific talents and resources away from basic research. This committee has heard this message time and time again. We Members of Congress who go to our respective universities hear the same message: The reason that the academia is focusing on the research projects in the military is because those are the only research projects there appear to be.

I think that we have to look very closely at what benefits the American industry can accrue, and that currently our R&D work concentrates much more on the military activities than does the work of our principal trading partners, and as we look at the investments of our trading partners I think that we will very clearly see that we must reshape our priorities.

This year, we are going to be faced with some difficult choices as we decide how to best invest our monies and these research dollars, and several major civilian R&D projects are under consideration as well as a great many smaller projects and very popular research programs.

So the best way to ensure, I believe, that we have a worthwhile civilian R&D approach is that we look toward a balance in achieving an overall Federal R&D effort that satisfies all the needs of this country.

Thank you, Mr. Chairman.

Mr. MINETA. Thank you very much.

Mr. Ritter.

Mr. RITTER. Thank you, Mr. Chairman.

I would like to associate myself with your remarks and, to some extent, my colleague from Rhode Island's remarks, only to add that we have an organization of R&D in this country which does not really permit the development of broad-based private sector industry-oriented breakthrough projects such as Sematech. If it wasn't for DOD funding, we wouldn't have a Sematech. Maybe we should have had it funded in another way, and I would agree, but it wasn't available at the time; the system was not set up. We wouldn't have a program, a nascent program albeit, in high-temperature superconductivity if it wasn't for DARPA. DARPA runs the Sematech program. We wouldn't have a nascent HDTV program if it wasn't for DARPA; DARPA runs the HDTV program. It has been the only game in town. Maybe that is something that Dr. Keyworth and others might wish to address.

Yesterday, I talked about what I call the $64 billion question—how the Federal Government can better set priorities for R&D and what we can do to better use our science and technology base to win global market shares in technology-intensive, high-value manufactured products.

Many believe that we have to begin focusing more on long-term national economic priorities that are technology based, that help America as a global economic power, just in the way we have focused on defense. We can get very long term about defense. It is time we did the same in some of the civilian technologies as well. But that means choices, hard choices, and choices even in basic research.

We need to ask ourselves a critical question: Is the organization of federal science and technology, this Federal R&D economy, largely within agencies whose missions reflect past priorities, up to the global economic challenge? I submit that it is not.

I am interested in the suggestions that our witnesses might have about a process of setting R&D priorities, particularly from our experts who have been in industry, who have been in government. The industry witnesses, in particular, have been fighting the global economic war, and, make no mistake about it, the challenge is enormous.

Our witnesses will shed some light on our ability to support breakthrough science and technology. Should there be, as I have argued, a process led by the President for guiding agency R&D, a kind of Team America approach, crossing the boundaries of past agency jurisdiction? Should we have a far greater coordination, integration, of the Federal R&D economy? I think it is essential. I think we have a chance to do it.

I think there is a window of opportunity with former governor John Sununu at the helm, probably the most qualified person in terms of science and technology at high levels in any Administration since Herbert Hoover, perhaps.

Should there be direct governmental support in some cases? What kind of consortia should we form? Should we target breakthrough technologies such as HDTV, biotechnology, advanced manufacturing science, photonics, opto-electronics, to promote their development?

Again, as I stated yesterday, answering these serious questions leads us to a reexamination of the role of the Federal R&D activity in stimulating our activity.

Finally, I would like to welcome our witnesses: my good friend Dr. Keyworth; among others, my constituent Larry Hecht of the Iacocca Institute at Lehigh University who will be testifying later who will join the industry panel. Larry is a real pro with vast industry experience in those global competitive battles and now is the Director of the Iacocca Institute at Lehigh University, a prominent player in thinking about and doing something about some of the issues before us today.

Thank you very much, Mr. Chairman.

Mr. WALGREN. The chair would like to thank the gentleman from California for starting the hearing and apologize for being delayed.

I would like to ask unanimous consent that any tape recordings or video recordings of this session be with the consent of the Members, and, without objection, that will be the order.

This morning, as has been said, we will continue our discussions of the adequacy and the allocation of Federal support for science and technology, and we begin with the consideration of the process in the executive branch for assembling the Federal research and development budget. Our particular interest is in the role played by the Office of Science and Technology Policy, especially the nature and the extent of its interaction with the Office of Management of Budget, and we welcome the comments of the distinguished witnesses that we have today regarding the recent recommendations of the national academies on modifying the budget process.

Also this morning, we will hear from managers and scientists who have had a long career in industry and considerable experience in making choices in allocating resources for research, and we are particularly interested in their views on the criteria that could be used to rank competing projects from different fields of science and technology. Also, we will appreciate having their thoughts regarding the adequacy and the direction of Federal support for civilian technology in particular.

We will have on the first panel Dr. George Keyworth, whom we welcome back from prior years, a former science advisor to President Reagan and the director of the Office of Science and Technology Policy, along with Mr. William Carey, whose long career in public policy aspects of science and technology has included service as an assistant director of the Bureau of the Budget, and we appreciate your coming back to our committee over the years.

To our second panel, we will be having Dr. Mary Good, the President of Engineered Materials Research at Allied-Signal Corporation; Dr. William Brinkman, the Executive Director for Research Physics at AT&T Bell Laboratories; and Dr. John Waymouth, a private consultant, formerly Director of the Research and Development Laboratories at GTE Lighting Group; along with Mr. Laurence Hecht, who is currently the Executive Director of Lehigh University's Iacocca Institute, as Congressman Ritter has pointed out.

Well, that is—those are our panels for this morning. Let me ask if there are other Members who would like to make opening statements.

I recognize Mr. Slaughter, if he would like to make an opening statement.

Mr. **Slaughter**. I have no statement, Mr. Chairman.

Mr. **Walgren**. Mr. Walker.

Mr. **Walker**. Thank you, Mr. Chairman.

I don't have an opening statement. I was just wondering whether Dr. Keyworth is still driving his Porsche.

Mr. **Keyworth**. I have a new one.

Mr. **Walker**. You have a new one? All right.

We got into a discussion some years ago and so on about our relative sports car loves.

Ms. **Schneider**. Dr. Keyworth, is it fuel efficient?

Mr. **Keyworth**. No. It was built in 1973 before we thought about that.

Ms. **Schneider**. All right.

Mr. **Walgren**. That is called a new old one.

All right. Let's begin, and, as you know from your prior appearances before the committee, your written statements will be made part of the record and incorporated in the transcripts that we create for those who work with these from a greater distance, and so you can count on that full explanation of all the points you might like to make being reproduced in the record. And please feel free to diverge from any prepared statements to elaborate or magnify points that you would like to underscore in our discussions.

So let's start with Dr. Keyworth.

STATEMENTS OF DR. GEORGE KEYWORTH, DIRECTOR OF RESEARCH, HUDSON INSTITUTE, INDIANAPOLIS, INDIANA; AND WILLIAM D. CAREY, FORMER ASSISTANT DIRECTOR, BUREAU OF THE BUDGET

Dr. **Keyworth**. Mr. Chairman and members of the committee, I won't exactly say that this feels like old times, but I am most pleased to see some familiar and maybe even a few friendly faces once again. I look forward to exploring this topic of priority setting with you, of course.

You know, today's discussion has its roots in a 44-year-old national policy written by Vannevar Bush, who was the architect of all post-war U.S. science policy. It was Bush who declared that basic research would be the breeding ground for America's economic growth and our progress. And the immense growth in Government support for university research through organizations like, first and foremost, DOD, NSF, NASA, NIH, all grew out of that one premise of Vannevar Bush gained from his experiences in World War II.

But we know that policies do age and they do fray. At the same time, they become entrenched, and after several generations of science done according to that model, I think we are bumping into issues and pressures that force some serious rethinking. The conflicts within the science community—the big versus little science, technology versus basic research—are but manifestations of those very pressures. I think we are in one of those awkward transitions, aware that we have to readjust to new conditions but justly cautious about tampering with success.

Now the conflict in the academic community today results from what I will call the rise of the technologists. The newly competitive industrial environment and the rapidly growing importance of technologies across the spectrum of science and engineering have caused two things; they have intensified competition for resources within the academic community, and they have begun to reestablish, after many decades, strong links between the universities' technologists and the commercial marketplace.

The strong pull of that external marketplace has elevated the importance and the ability to command resources of the technologists, and it has led to a new vulnerability for basic research, long protected under Vannevar Bush's visionary foresight.

Now to my mind, that qualitative conflict is the essence of the problem, and we need a new science and technology policy that balances those conflicting pulls, not by looking for consensus and trying to please everyone but by responding to the most important and the best opportunities in this broader spectrum.

Now nurturing and protecting basic research is the easier part of the problem, and we've had lots of experience doing just that. It's the technology part of the policy requirements that still makes us uncomfortable and which we haven't adequately addressed yet. That's why I've been disappointed over the past year with the misplaced emphases in discussions about science and technology policy.

In my opinion, the inordinate amount of attention paid to things like the status of the science advisor or the number of people on his staff have wasted intellectual and political capital. There's only so much attention that can be given to any topic, and we simply shouldn't waste it on things that are mostly show and very little substance.

In looking at how the existing process works, let's be careful to make sure we separate weaknesses in ability to set priorities from personal dissatisfaction with the priorities that, in fact, do get set. When I was science advisor, my staff and I had an active, and I think effective role in setting priorities for R&D programs. We looked at the national priorities on the one hand and the agencies' programs and resources on the other, and we worked hard to find an effective match-up.

At the same time, some scientists didn't like the administration's emphasis on using science and technology to strengthen industrial competitiveness and national defense—but that was an expression of the President's priorities, and I can't see how giving the science advisor a fancier title or appointing a body of visible outside advisors would change that very much.

However, having said that, I will endorse the idea that we didn't and we don't go anywhere near far enough. Optimizing the R&D programs of individual agencies is a necessary but not sufficient step to optimizing a national overall R&D program. I'd like to tell you what I think should be driving our discussions of priorities and of science and technology policy, and it is not the relative value of the SSC and the space station—one of which represents a national commitment to excellence and leadership in research and the other of which is an investment in neither excellent science nor excellent technology. It is not deciding if small science should get some of big science's money or vice versa. It is not the relatively self-serving issue of how healthy the science establishment is, nor is it the number of associate directors in OSTP or how well the science advisor is respected by the bureaucrats in the various agencies. Those are not the important issues.

The central issue is that in our country the Federal Government spends about $65 billion, or half of all the money spent in the United States, on science and technology, on research and development. For all practical purposes, that means that the work of about half of all the scientists and engineers in this country is determined by Federal policies for science and technology. That is an enormous resource, and, to my mind, it is the single most important resource we can bring to bear in dealing with the serious challenges that we are confronting in industrial competitiveness which has marched through the eighties to become the most serious threat our country now faces.

But we are not doing it very well. And what's not working right, I believe, is the culture: A culture in academe that often sees capitalism as counter-creative; a culture in Government that sees business as threatening its very prerogatives; and a culture in business that is skeptical of both Government and of universities. Until this set of adversarial relationships is reversed to the way it was in Vannevar Bush's post-war time, no amount of attention to funding priorities will achieve the real desired objective, which is using science and technology better to preserve our way of life.

Now for the past four or five years, I confess to have been poking sticks in hornets' nests by daring people to think about a Department of Science and Technology, and I've done that for a couple of reasons. The first is simply to raise the priority—to focus attention, if you wish—on that half of America's total R&D that Government policies dictate. The current diversity of R&D direction in the Government is simply incapable of changing fast enough to meet the Nation's need whether in education, in space, in defense, in biomedicine, or in supporting our industries trying to compete against those nations that do use their resources more effectively than we do.

But the second reason I've proposed that we think about a Department of Science and Technology is to shine a harsh light on the very same issue that we are discussing today, priorities. We currently do our trade-offs during budgeting on a small scale. By putting together a coherent program out of a single budget, with visible means of accountability to the Congress and to the public, and it would force us to make the kinds of trade-offs that are sometimes avoided in this current diversity we have now.

I predict that a great deal of effort in this slippery world of science and technology policy in coming years will be devoted to some new kinds of issues. In the United States, there is a disturbing gap in the continuum between basic research and products on the shelves. It's becoming painfully obvious that there's a big part of this kind of applied research which is mandatory for transforming knowledge into technology, that we simply do not any longer do well. Or, at least we don't do it as well as some others do.

I've been observing this personally in the area of high-temperature superconductivity, a field of immense promise, but where the academics, the Federal labs, and industry all seem mutually reluctant to step into that no man's land of pre-commercial generic technology development, device development. It's a no man's land because it's too applied for the academics, too commercial for the Federal labs, and too far from market for industry.

Government is going to find itself hip deep in this problem very soon. We're going to have to develop new mechanisms to populate that barren ground, and that may turn out to be the most significant issue we deal with in science and technology priorities. The early manifestations are already there. Sematech, as Congressman Ritter has already referred, represents a real departure for Government from the kinds of hands-off stance it has generally taken with regard to industry's interests. But Government concluded that the semiconductor industry which had grown out of U.S. basic research was simply too important to our future to ignore. And similar conclusions are emerging about superconductivity and high-definition television.

But let me finally pose one question: Why, in each of those cases, has it been the Department of Defense, DARPA in particular, that has ridden to the rescue? And doesn't it underscore the concern I raised earlier about our existing system of support for science and technology simply not being up to the task of addressing those fundamental technology issues that are critical to our national well-being?

Mr. Chairman, that concludes my testimony, and I hope I might have provoked a question or two later.

[The prepared statement of Dr. Keyworth follows:]

Proposed Testimony of Dr. G. A. Keyworth, II
Director of Research
Hudson Institute
Indianapolis, Indiana

To the Subcommittee on Science, Research and Technology
Committee on Science, Space, and Technology
United States House of Representatives

Hearings on Priority Setting in Science and Technology
March 1, 1989

Mr. Chairman and Members of the Committee:

I won't exactly say that this feels like old times, but I'm pleased to see familiar, and maybe even a few friendly faces once again. I look forward to exploring this topic of priority-setting with you.

Today's discussion has its roots in a 44-year-old national policy written by Vannevar Bush, who was the architect of post-war U.S. science policy. It was Bush who declared that basic research would be the breeding ground for America's economic growth and progress. The immense growth in government support for university research, through organizations like DOD, NSF, NASA, and NIH all grew out of that premise.

But policies do age, and fray, at the same time they become entrenched. And after several generations of science done according to that model, we're bumping into issues, and pressures, that force rethinking. The conflicts within the science community--the big versus little science, and technology versus basic research--are manifestations of those pressures. We're at one of those awkward transitions--aware that we have to readjust to new conditions, but justly cautious about tampering with success.

The conflict in the academic community today results from what I'll call the rise of the technologists. The newly competitive industrial environment, and the rapidly growing importance of technologies across the spectrum of science and engineering, have caused two things. They have intensified competition for resources within the academic community. And they have begun to re-establish, after many decades, strong links between the universities' technologists and the commercial marketplace. The strong pull of that external marketplace has elevated the importance, and the ability to command resources, of the technologists, and it has led to a new vulnerability for basic research--long protected under Vannevar Bush's visionary foresight. To my mind, that qualitative conflict is the essence of the problem, and we need a new science and technology policy that balances those conflicting pulls--not by looking for consensus and trying to please everybody, but by responding to the most important and the best opportunities in this broader spectrum.

Nurturing, and protecting, basic research is the easier part of the problem; we've had lots of experience doing so. But it's the technology part of the policy requirements that still makes us uncomfortable, and which we haven't adequately addressed yet. That's why I've been disappointed over the past year with the misplaced emphases in discussions about science and technology policy. The inordinate amount of attention paid to things like the status of the Science Advisor, or the number of people on his staff, have wasted intellectual and political capital. There's only so much attention that can be given to any topic, and we shouldn't waste it on things that are mostly show and little substance.

In looking at how the existing process works, let's be careful to make sure we separate weaknesses in ability to set priorities from personal dissatisfaction with the priorities that, in fact, do get set. When I was Science Advisor my staff and I had an active, and I think effective, role in setting priorities for R&D programs. We looked at the national priorities on the one hand, and the agencies' programs and resources on the other, and we worked hard to find an effective matchup. At the time, some scientists didn't like the Administration's emphasis on using science and technology to strengthen industrial competitiveness and national defense--but that was an expression of the President's priorities, and I can't see how giving the Science Advisor a fancier title or appointing a body of visible outside advisors would change that.

However, having said that, I will endorse the idea that we didn't, and we don't, go anywhere near far enough. Optimizing the R&D programs of individual agencies is a necessary, but not sufficient step to optimizing a national R&D program. I would like to tell you what I think should be driving our discussions of priorities and of science and technology policy. It is not the relative value of the SSC and the Space Station--one of which represents a national commitment to excellence and leadership in research, and the other of which is an investment in neither excellent science nor excellent technology. It is not deciding if small science should get some of big science's money or vice versa. It is not the relatively self-serving issue of how "healthy" the science establishment is, nor is it the number of Associate Directors in OSTP or how well the Science Advisor is respected by the bureaucrats in the agencies. Those are not the important issues.

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But we're not doing it very well. What's not working right is the culture: A culture in academe that often sees capitalism as counter-creative; A culture in government that sees business as threatening its prerogatives; and a culture in business that is skeptical of both government and universities. Until this set of adversarial relationships is reversed, to the way it was in Vannevar Bush's post-war time, no amount of attention to funding priorities will achieve the desired objective--using science and technology better to preserve our way of life.

For the past four years I've been poking sticks in hornets' nests by daring people to think about a Department of Science and Technology. I've done that for a couple of reasons. The first is simply to raise the priority--to focus attention, if you wish--on that half of America's R&D that government policies dictate. The current diversity of R&D direction in the government is simply incapable of changing fast enough to meet the nation's needs, whether in education, in space, in defense, in biomedicine or in supporting our industries trying to compete against those nations that use their resources more effectively than we do. The second reason I've proposed we think about a Department of Science and Technology is to shine a harsh light on the same issue we're discussing today--priorities. We currently do our tradeoffs during budgeting on a small scale; but putting together a coherent program out of a single budget, with visible means of accountability to the Congress and to the public, would force us to make the kinds of tradeoffs that are sometimes avoided in the current diversity.

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But let me pose a question: Why, in each of those cases, has it been the Department of Defense, DARPA in particular, that has ridden to the rescue? And doesn't it underscore the concern I raised earlier about our existing system of support for science and technology not being up to the task of addressing those fundamental technology issues critical to our national well-being?

Mr. Chairman, that concludes my testimony. I hope I have provoked a question or two.

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Mr. **Walgren**. Thank you very much, Dr. Keyworth, and we'll come back, and we'll turn to Mr. Carey at this point and get you both on the record.

Mr. **Carey**. Thank you, Mr. Chairman and members of the committee. It's a pleasure to see you all again.

I have to apologize for the condition of my larynx this morning, and if my voice breaks I don't want you to think I'm overcome with emotion.

My brief testimony will be based on my Government service in the predecessor OMB for a very long time and 12 years as chief operating officer of the American Association for the Advancement of Science.

This invitation asks me to address a number of tough questions, and I'll see what I can do with them, but I don't think categorical answers are all that clear.

First, as to the adequacy and balance of the national science and technology effort, my line of sight makes me think that, in the aggregate, a combined public and private current outlay level of about $130 billion a year is a very high level of effort. If we project only modest increases, it works out over the decade of the nineties to a combined outlay of at least $1.5 trillion of public and private funds going into R&D, and I'm not about to call that inadequate.

Your question, on the other hand, about balance gets a different response. I think we should be greatly dissatisfied at the widening gap between the opportunities that we have in science and our ability to fund more of them, and one arresting piece of evidence along this line is the declining rate of funding of research proposals in basic science and the biomedical sciences where only about 22 percent of highly rated proposals can be supported with the money that is now available. And what this means, as I see it, is that we are, in effect, disinvesting in the stock of research at the very time that we are talking about competitiveness, and competition. And trouble—and this, I think, is a signal that could go a long way to explain why, for the first time, we're seeing phenomena like a tailing off in total applications going into NIH and a marked disinclination by our brightest students to opt for advanced degrees in science and engineering.

You see trends like this, and you have to start asking questions about how much productivity are we going to be able to count on from a $1.5 trillion outlay on science and technology in the nineties. That's a critical question, I think.

My only other comment on adequacy and balance is a very brief observation that the national technology base both for civil and defense requirements is at no small risk on a number of counts, among them the poor quality of pre-college math and science preparation, growing dependence on foreign sources of innovation, and disarray in the management of the defense technology base. Time won't allow me, neither will my voice, to speak further to this, but I understand that the OTA is shortly going to release a major report that will go into this set of problems of the technology base, particularly the defense technology base. And I hope that your subcommittee and the Armed Services Committees can get together to follow up on it; it's a major window on the kinds of problems we have.

You ask about setting priorities and the process for it in Government. You ask if the Government is organized suitably. I don't think the organization of the Executive Branch is the key to the priorities dilemma. We simply have not had a framework for rationalization that is grounded to a multi-year strategy and a financing plan that enables that strategy to work. It's an institutional problem. In part, it grows out of science's natural resistance to being programmed and prioritized, and it's a well founded resistance—as well as, in equal part, Presidential and Congressional preference to keep open the budget options in the outyears—and that's something that we have to reckon with.

In this connection, I want you to know that I do take a favorable view of the ideas that have come out of the leadership of the National Academy of Sciences recently. The thrust being to sort out R&D funding proposals in a four-dimensional structure, instead of the incoherent and nonaggregated presentation we now follow. And beyond suggesting a better analytic presentation the Academy leadership called for a collaboration between OMB and the White House Science Advisor to develop cross-cutting R&D objectives as planning guidance to the agencies, and I'm in favor of that. In addition, it calls for reviews of cross-cutting activities by the Budget and Appropriations Committees of the Congress before the budget is broken down and fed in pieces to the authorizing and appropriation committees.

I'd have two observations to make about this process reform. First, I know of no reason why the changes suggested for the Executive Branch side wouldn't work or shouldn't work; I know of no reason at all; they would be a definite plus. Second, I see a much tougher, much more difficult adjustment being asked of the Congressional machinery, although there is a lot of logic for the suggestions.

It may well be that to keep—I guess what I am thinking about is a time in the past when the Appropriations Committees and the President agreed on a new system for getting the budget issues resolved, and it was a lot of paper, a lot of structure, and a lot of hand-shaking, and it broke down after a year. That was when Mr. Taber was chairing the House Committee on Appropriations; it goes way back. But I bring it up because I think that, to keep the legislative procedures that are suggested here from being abandoned after one shot, Congress, I think, should be willing to adopt a two-year authorizing and appropriating cycle for R&D, sort of as to build enough time into the process to make it workable. I'm not talking about giving research more social security with a two-year authorization and appropriation, I'm talking about making this Congressional-Executive collaboration workable so that it simply doesn't collapse out of everybody's frustration.

And, finally, you want comment on the interactions of OMB with OSTP and budgeting. Well, the first thing that has to be said, I think—I don't know if Jay would agree with this—I must say that it's a pleasure to see him again and just offer the comment that he's received much too little credit for all that he did for basic science in universities during his White House period.

The first thing that has to be said about the OMB-OSTP interaction in budgeting is that this interaction is dominated by the policy-making seniority of the Presidential budget, which is what sets priorities and expectations that suit the program of the President; that's what the budget is, it's not developed in consultation with Heaven, it's developed to carry out and effectuate and institutionalize the President's goals.

Now this makes the science and technology input secondary rather than equal, and the funding dispositions for R&D that come out of it are expected also to fit into the program of the President, and we're not going to see that changed, in my opinion.

But within these general stipulations, I will say that OMB needs the technical expertise of OSTP in constructing outcomes that in fact will constitute both good science and technology policy and articulate Presidential preferences and support.

I think that the statements that President Bush has made recently, particularly in his appearance before the Congress, are most encouraging. They do tell us where the new President's mind is. What we've got to do, I think, is so arrange the processes and put some flexibility and stretch into them so that they don't all get caught in one dreadful crunch in the annual budget.

I can't speak to the quality of the cooperation between OSTP and OMB since I left Government a long time ago, in early 1969, but I suspect that it has gone on with both high and low points, and we know what some of them were without my going into them.

I think it's terribly important, although Jay didn't seem to think so, that OSTP have adequate staffing. You know, you are talking $64 billion, and you're talking about a line of sight over the industrial area of R&D, and you don't do this on the run. It's very, very hard to grasp the architecture when you're chasing down daily staff memos and answering telephone calls and doing all the other things that a handful of people have to do in the President's system. And we just don't have the reach that we need. And I think adequate staffing is necessary in OSTP. And I also think that the other critical condition in the relationship is a seat at the OMB's budget review table for the principal officers of OSTP, and that's the minimum that we should expect. And I think that the willingness of OMB through—my goodness, 40 years of dealing with scientific matters, the willingness of OMB to shelter basic research through presidencies that have at times been adverse to basic research, not utilitarian enough, is a remarkable thing that OMB deserves credit for.

I think that what I've heard this morning so far, the arguments about shaping up research and development all through the system and charging that the problem is of international competitiveness—with all the spears sharpened to a fine point—is all very well, but I don't think you can take science and make it march in a programmed or overprogrammed direction. I think you have to be very, very careful that we don't overdo it. It's a risk, I think, that is inherent.

When I heard Jay speak of Sematech and the superconductivity problems and how they came out of DARPA in terms of at least getting something started—although not much has followed—it takes me back to a view I expressed a couple of years ago that I would like very much to see Government look at these questions that have been raised from the standpoint of why the system doesn't come together the way it should. And I think part of it is that we have no way of articulating Government's efforts in R&D with efforts of industry. The system pushes past each other like tectonic plates.

There has to be more articulation, but it has to be done in a framework of not too much government, and I have suggested a civilian ARPA, if you will, not with billions of dollars to spend but situated in the Executive Office, possibly as an alternative to an OSTP, that would have the job, with the President's backing, of making the Government system and the industrial system fit a little better. And when you look at the National Laboratories—again, Jay has said that the laboratories think that superconductivity, for example, is too commercial to play with.

I am on the Board, for my sins, of the Argonne National Laboratory, and I know the enthusiasm of that Laboratory to pursue superconductivity imaginatively and creatively. And it's not an attitudinal question there, I can tell you, it may very well be a resources question.

Well, Mr. Chairman, my voice has lasted longer than I thought it would, and I'll leave it at that.

[The prepared statement of Mr. Carey follows:]

Testimony by William D. Carey
(Formerly Assistant Director, Bureau of the Budget)
on
The American Science and Technology Effort

Subcommittee on Science, Research and Technology
Committee on Science, Space, and Technology
U.S. House of Representatives
March 1, 1989

Mr. Chairman and Members of the Subcommittee:

My brief testimony this morning will be based upon my government service in the predecessor OMB, for quite a long time, and my twelve years as chief operating officer of the American Association for the Advancement of Science.

Your invitation to testify asked me to be prepared to cover a number of formidable questions, and I shall respond as best I can, although categorical answers are not obvious.

First, as to the adequacy and balance of the national science and technology effort, my line-of-sight makes me think that, in the aggregate, a combined public and private current outlay of around $130 billion a year constitutes a very high level of effort. Projecting only modest increases, it works out over the decade of the Nineties to a combined outlay of at least $1.5 trillion, and I'm not about to call that inadequate. Your question about balance, on the other hand, gets a different response from me. I think we should be greatly dissatisfied at the widening gap between the opportunities in science and our ability to fund more of them, and one arresting piece of evidence along this line is the declining rate of funding of research proposals in basic science and the biomedical sciences, where only about 22 percent of high-value proposals can be supported with the money available. What this means is that we are, in effect, disinvesting in the stock of research, and this is a signal that could go far toward explaining why we see, for the first time, such phenomena as a tailing-off in total applications to NIH, and a marked disinclination by our brightest students to opt for advanced degrees in the sciences and engineering. With trends like that, we should start asking questions about how much productivity we can count on from the $1.5 trillion outlay on science and technology in the Nineties.

My only additional comment on adequacy and balance is a very brief observation that the national technology base for both defense and civil requirements is at no small risk on several counts, among them the poor quality of precollege math and science preparation, growing dependence on foreign sources of innovation, and disarray in the management of the defense technology base. Time will not allow me to speak further to the state, and prospects, of the tech base but I understand that the OTA will shortly release a major report that will go into this set of problems very thoroughly and I hope your Subcommittee and the Armed Services Committee can follow up on it.

Your next question concerns the process for setting priorities for science and technology. I do not think that the organization of the Executive Branch is the key to our dilemma of priorities. We simply haven't had a framework for rationalization, grounded to a multi-year strategy and a financing plan consistent with it. This is a flaw in process, not in organization. It's an institutional problem, and in part it grows out of science's natural resistance to being programmed and prioritized, as well as, in equal part, presidential and Congressional preference for keeping open the budget options for the "out" years. In this connection, I take a favorable view of the proposals made recently by the leadership of the National Academy of Sciences, the thrust of which is to suggest sorting R and D funding proposals into a four-dimensional structure instead of the incoherent and non-aggregated presentation that we now follow.

Beyond suggesting a better analytical presentation, the Academy calls for collaboration - between OMB and the President's Science Advisor in developing cross-cutting R and D objectives to serve as planning guidance to the departments and agencies. In addition, the proposal calls for reviews of cross-cutting activities by the budget and appropriations committees before the budget is broken down and fed to the authorizing and appropriation committees. I would have two observations to make about this restructuring of process. First, I see no reason why the procedural and substantive changes suggested on the Executive Branch side wouldn't or shouldn't work. They would be a definite plus. Second, I see a much more difficult adjustment being asked of the Congressional machinery, although the logic for the proposals is self-evident. It may well be that, to keep the legislative procedures from being abandoned as inoperable, Congress will be willing to adopt a two-year authorizing and appropriating cycle for R and D, to build enough time into the process to make it workable.

Finally, you ask for comment on the interaction of OMB with OSTP in the budgeting process. The first thing that must be said is that this interaction is dominated by the policy-making seniority of the presidential budget, which sets priorities and expectations that fit the program of the president. This makes the science and technology input secondary rather than equal, and the R and D funding dispositions are expected to likewise fit the program of the president. We are not likely to see that changed. Within these general stipulations, I will say that OMB needs the technical expertise of OSTP in constructing outcomes that will, in fact, constitute good science and technology policy as well as articulate presidential preferences and support. I cannot speak to the quality of the cooperation between OSTP and OMB since I left government in 1969, but I suspect that it has had its high and low points. What is very important in this relationship is (a) adequate staffing for OSTP, which it has not been getting, and (b) a seat at the OMB's Budget Review Table for the principal officers of OSTP, and this is the minimum that we should expect.

That concludes my prepared statement.

William D. Carey

Mr. Carey is recently retired as Chief Executive Officer of the American Association for the Advancement of Science, a post he held from 1975 to 1987. Prior to heading AAAS he was vice-president of Arthur D. Little, Inc., following a long career in the Bureau of the Budget, Executive Office of the President as Assistant Director.

He is presently Consultant to the President of the Carnegie Corporation of New York and a trustee of the MITRE Corporation and the National Academy of Public Administration. He is also a member of the Board of Governors of the Argonne National Laboratory and trustee of the Scientists Institute for Public Information. In addition, he is a member of the Council on Foreign Relations and the Scientific Advisory Committee to the State Department's Bureau of Oceans and International Scientific Affairs. He is an Honorary Member of Sigma Xi and the U.S. National Academy of Sciences, and an ex-officio member of the Governing Board of the National Research Council. He is an alumnus of Columbia and Harvard, and his field of education was in public law and government.

Mr. **WALGREN**. Thank you, Mr. Carey.

Let me pursue that point with you. I can remember in 1977 when we were in the energy crisis Tom Harkin had a proposal in this committee—and we had General Motors in front of us—and the proposal was to pursue a high-mileage engine. And General Motors saying, "Well, we don't mind if you do a little bit of this and a little bit of that, but we don't want you putting any nuts and bolts together because that's what we do."

Then John Young, in the National Competitiveness Council, I remember saying that the problem was one of a lack of legitimacy on the part of the Federal Government's efforts in pursuing advances in industrial competitiveness. And Jay Keyworth says that—where the heck was it, but it's a culture in Government and business that sees it's threatening its prerogatives, and until that adversarial relationship is reversed you foresee little progress.

Everybody who gets involved in science and technology with a feeling of responsibility comes away saying that we are being prevented from doing things that we should be doing as Government because of ideology, because of some philosophy of limitation of Government. Is there some way that we can take that issue head on in hopes of getting by it a little bit more than we apparently are now?

Let me ask Dr. Keyworth, and I guess you have suggested taking it head on by establishing a civilian ARPA.

Mr. **CAREY**. Yes, I will be glad to comment further on that, Mr. Chairman.

Mr. **WALGREN**. But somehow or other we have got to get at that. Or we're just blocked until a national security need by semiconductors comes up, and the Defense Department will do something. But we've missed all the interim opportunities.

Dr. Keyworth.

Mr. **KEYWORTH**. Mr. Chairman, first of all, I'm delighted because I think that you have highlighted the very core of this problem.

Now you know we have been consumed for years over this discussion of technology transfer, and I think one of the most astute statements I have heard on this subject referred to it as a body contact sport. You know, until we are able to identify common problems that people from industry and people from academia and people from the Federal Government have a common interest in developing a solution to—we will not achieve the kind of unity that served us so well in World War II.

We must remember—and we often forget the history—that American science, academia, and Government, and industries worked together like brothers during World War II, and the progress that we made and the speed with which technology went from an idea to a tangible solution was absolutely remarkable. It continued for a certain period of time after the war. What we have lost in this extended period, fortunately, of peace has been the ability to identify common problems. Sematech, perhaps HDTV, and perhaps superconductivity are beginning kernels of those.

The value, to me, is more that they represent things that each segment sees as important from its own perspective than the individual thing itself.

Mr. **Walgren**. Let me just take you back. You say what we've lost is the ability to identify common problems. That seems to be worth underscoring, and perhaps we can think of approaches that might get to that. I don't mean to cut you off. I just—when something makes sense to me, I like to flag it.

Mr. **Keyworth**. I think it's a key point.

Let me just make one comment, if I may, on this concept of a civilian DARPA, because I think maybe we'll have some good discussion on this. You see, I don't think we're all talking about such different things. I also believe that there needs to be a central figure in the Executive Branch with authority and implementation capability to identify these common problems and bring together the resources and the talent to deal with them.

My argument is that if it is a DARPA—at the level of DARPA, at the fourth tier in a Department—it will have very little effect.

Now DARPA's situation today is unique. First of all, if I may say, DARPA has had a few very rare heroes in the Federal bureaucracy. There are a few people who have risked their reputations and their careers in order to pursue something that they deeply believed in. And they have been extremely well supported more by the Congress than they have by the Defense bureaucracy itself, and I hesitate to believe that that can be repeated with ease.

I believe that the purpose that a civilian DARPA would pursue would be the primary change in bringing our diverse R&D efforts together into a single, central, focused point. But I really want to emphasize the fact that we are confusing real power with the illusion of power. There is no real power, no real ability to make change, without some strong ability to allocate resources directly. And I believe that when we look at a Department of Science and Technology, and elevate it so it will be elevated science advisor—when I caution you that the Congress has already made the science advisor the same formal rank as the President's chief of staff, so don't look to rank—when we think about this, we are going to have to come back to the brass tacks here, which is, you need some mechanism to identify a common problem, and you need the genuine authority, not the illusion of authority, to carry that out.

Thank you.

Mr. **Walgren**. Thank you.

Let me give Mr. Carey time to respond, and then I want to recognize Ms. Schneider.

Mr. **Carey**. I think Jay and I see this thing very much the same way. Although there are some marginal differences. Jay puts his money on a Department of Science and Technology, and I wouldn't bet a dime on it. And the reason why I say that is that I think, again, it's a red herring, with all respect.

The problem is the quarreling that would focus around the architecture of a Department of Science and Technology. I don't believe for a second that the military would allow its R&D to be part of such a Department. I don't believe that the Biomedical Industry in this country would, indeed, put its fortunes at risk in such a Department.

I think that what we'd wind up with would be a kind of a crippled representation of something that we would call a Department, with a whole lot of protected cells and earmarking of appropriation and everything else, and I don't think it would be administrable or effective.

I think I much prefer to get at the problems we're trying to get at by being lighter on our feet and not dragged into the routines and the rituals of Government Departments as such.

Mr. **WALGREN**. Let me ask you this if I might. And that is, that perhaps what we need is some call to recognize the legitimacy of the Federal effort in this area.

I remember looking at the National Bureau of Standards, which has worked most prominently in the Federal Government on this level, and when you scratch them, they are doing interesting things in a pragmatic way. But when you ask them what they are really doing, they say, "Oh, we're just in here measuring. Don't mind us. We're just doing some measuring research here under the old charter of 1871," or something like that.

The point is that what isn't appreciated is the legitimacy of the Government being whatever it can be. And maybe we should be talking about that, but when we don't have the legitimacy of the effort established, we're very far from being able to carry it out.

Well, I want to apologize for going on and recognize the gentlelady from Rhode Island, Ms. Schneider, and ask the gentleman from California if he wouldn't take the chair and excuse me in the process.

Mr. **BROWN**. Whatever you say, Mr. Chairman.

Ms. **SCHNEIDER**. I certainly appreciate the comments of both of our witnesses this morning, and, Dr. Keyworth, please understand that I appreciate your tangible recommendation as a solution in the form of the Science and Technology Department.

But I am not going to confine my questions to that tangible solution, because I happen to believe that we don't have the time nor the energy that would be required and involved in the bureaucratic hangling to put together such an effort. I happen to believe that, given our economic stature in the global community at this point, given the other scientific challenges that we have, such as AIDS and global warming and a variety of other things, that we need to set our priorities ASAP, and I think that, given the various agencies that have jurisdiction over some of those research initiatives, certainly they ought to be in a position to do that.

I'd like to ask you, however, about, briefly, some of your thoughts about the 83 percent increase that we have seen in R&D spending over the last eight years, 83 percent, of course, going to the military, and are there not trade-offs that are being forced to occur as a result of that kind of priority setting?

Mr. **KEYWORTH**. I do have serious concerns about the trends that have occurred during that 83 percent, but I also must say that the distinction between defense and civil, I think, is, to use the words of Bill Carey here, red herrings. And let me cite a few facts.

First of all, we must remember that some years ago the organization that built the foundations for American excellence in science was the Department of Defense.

Ms. **SCHNEIDER**. Mr. Keyworth, excuse me. You, yourself, said at the opening of your statement, which was a piece of wisdom that jumped out at me and landed right in front of me—was that our policies have aged, they have frayed, they have become obsolete.

Those, I believe, were your own words, and it seems to me that, is that not where we are right now in terms of—

Mr. **Keyworth**. If I could complete my thought, perhaps I'd be able to make the point.

Ms. **Schneider**. Okay.

Mr. **Keyworth**. It's not the defense issue alone. What has happened is that R&D is a very broad spectrum, and we have seen too much D and too little R.

If we think of it as a simple model, research at the most basic end trains the next generation of scientists and engineers. We have created demand for scientists and engineers throughout the Government, including in defense, and we have not paid enough attention toward the—this is the seed corn argument, of course, and I feel that there has been a serious problem there.

The Department of Defense I would fault for not having paid enough attention to its own responsibility in terms of fundamental research.

But this overarching question that we are trying to deal with here I think is captured best in our ability to move to compete in such areas as high-definition television, to develop its—yes, AIDS is a challenging problem, but in a way the horizons that are available to people in biomedical research are taking this catalyst of urgent concerns about AIDS to make monumental breakthroughs in the treatment of virally caused disease and understanding of the entire immune system.

The problems are well identified. The challenge to me is that we are not able; to identify the common problems; recognize industry, university, and the Federal Government's responsibilities; and spend our resources in a way that both deals with those problems in a coherent way and, secondly, takes the long-range perspective that assures that we will have the people to carry those out and to deal with the next one.

Ms. **Schneider**. I have a problem in that just now you said that the problems have, in fact, been identified, but earlier—and Chairman Walgren had repeated what you had said, that we do have an inability to identify our problems. So now I'm lost.

Mr. **Keyworth**. I think the areas of particular technical opportunity today, scientific and technical opportunity, are out on the table. I don't even think there is very much dispute about them, which is one of the concerns I have about the whole priority setting issue. We want to return to American excellence in space. We want to be major players in the information age. And the high-definition television issue is really an issue of how we go forth to integrate America's digital telecommunications system which we have let flag for a decade. AIDS is a critical issue and one to which we can genuinely, I believe, see our way down the road to a real solution, and the impacts will be huge.

I could go on and on. Those problems are there.

The real problem that is not on the table is the way by which we restore the kind of partnership that we have seen historically in the country and which we do not any longer have between our segments. That is the problem to which I do not believe we have come to grips.

Ms. **Schneider**. Well, one of the things that—I agree with some of the comments that have been made, to some degree obliquely, that perhaps we are, in fact, spending enough money on research overall, but what is critical is to determine where we spend more and where we spend a little less, and you had also mentioned in your comments or made a reference to our educational system and our reliance on foreign engineers, and all the way through the system it seems that we have lost some priority-setting there.

Well, it seems to me that we have, as you say, identified those problems. If we maintain the same amount of money for research and technology, for science research and technology, then clearly we have to determine what is truly important in terms of where we allocate a few more monies, and I think part of that has to be done coming from the grass roots. There has to be both a bottom up and a top down.

There is a reason why we have fewer students pursuing engineering and sciences, and I have been trying to get to this question for years now. It's just not because children have not had adequate education in the primary schools for science, it is because children are getting to their senior years in college and deciding they don't want to go on to graduate school because the only job opportunities that appear to be available are in the defense sector. I have heard this repeatedly from graduate students, and to me it is most disconcerting. So I think that we have to look at what kind of message as policy setters we are actually putting forward.

Now if you were to go to a class of students and tell them that scientists today are tracing the increasing of our earth's—of the planet earth's temperature and that they could play a role in helping to minimize that, either to identify the science or to help define the impact or to provide solutions, I think that would even capture the imagination more than recognizing that the message that many of these students are getting is one that is more destructive than creative.

Mr. **Keyworth**. Ms. Congressman, being the father of two teenagers, I wish that all their decisions were based upon facts, but they, even more than we, I think, tend to use perception.

You know, there are a few facts here. One is that only one-third of the R&D conducted in America—or, I should say, really, as much as one-third of the R&D conducted in America, but only one-third, is for defense, and it is not the fastest growing sector. In fact, the National Science Foundation has grown faster during the last eight years than the Defense Department has. We must not confuse total national R&D, which is the employment base, versus Government alone. It's only a third.

Sectors like the pharmaceutical sector, the information technology sector, the advanced materials—advanced and engineered materials sector are enormous sources of employment. And if people, young people, believe that the defense sector is the place for opportunities, they are simply overlooking the facts.

Ms. **Schneider**. But you were talking about perception and reality, and I do believe that it is the responsibility of the Federal Government to convey the facts, and it seems to me that they have a perception for a particular reason. So, granted, that is our responsibility.

Mr. KEYWORTH. I agree with that.

Ms. SCHNEIDER. Thank you, Mr. Chairman.

Mr. BROWN [presiding]. Does the gentleman from California have any questions?

Mr. MINETA. Thank you, Mr. Chairman.

We see, or I heard, commercialization; I've heard short range, long range; I'm still not sure how we establish broad national goals. How best should that be done? How do we sift that out?

Mr. KEYWORTH. I believe, myself, that the broad national goals are being set by public demands. The broad national goals in this country—and I won't define some of the same ones before—are clearly in the health sector. There is an urgent need to deal with diseases of the immune system and to gain a better understanding, and a substantial fraction of the entire talent in America available to do that is actually out working on it, regardless of what budget items say; that's a fact.

Secondly, this competitiveness issue is not one that has been well defined. That is, if you asked ten people out on the street what competitiveness means, they will use different ways to describe it. But basically what it means is that other nations are beginning to surpass us in the areas where we believe future jobs and opportunity lie. And they are surpassing us by making better products at more competitive prices than we are.

So somehow we need to couple our scientific expertise better to that industrial economic machine. That's a problem. The solutions are what have not come out on the table. The solutions—what does the Federal Government do? where does it concentrate its resources?—and if I'm being difficult by avoiding some of the intentions of any of the Members' questions, it is because I simply do not believe that arguing over whether SSC or the space station or engineering research centers or Sematech is the best place to spend money—it will help us to—it will help us over the next six months, but it will not help us in any significant way to rectify this fundamental problem that we are losing our ability to exploit technology in spite of the fact that we maintain—continue to maintain in the United States a dominant, an overwhelmingly dominant capability to generate new ideas and new creative people.

Mr. CAREY. If I could just make a comment, Mr. Chairman, with what's left of my voice, last night on my way home I was reading part of Sunday's New York Times, and the item that struck me had to do with tourism—you may have seen it—and the remark was that tourism today is the third largest industry in the United States economy, but that the Commerce Department is telling us that by the year 2000 tourism will be the number one industry in the American economy. And I think that while this is gratifying in a world that looks on leisure as its highest value, it's a little disturbing to those of us who now see a GNP that is a very soft GNP, that is very strongly consumer dominated and very weak in terms of solid fiber.

I think the business of priorities setting in an open society has got to be a constant dialogue and debate, and it needs a lot of information. I think we look at science and technology, and—you know, Dr. Brinkman is here, and I can remember the Brinkman report on physics—opportunities in physics—coming up in the National

Academy of Sciences two or three years ago. And it was a whole feast—a whole feast of creative opportunity, and then, shortly after that, we had a somewhat similar exercise in chemistry, and you can go right over the disciplinary spectrum, and the opportunities are simply blinding. And yet, as I said a little earlier, when we, as Government, with a trusteeship for basic science in this country, like it or not, old-fashioned or not, and Government is able only to fund one-fifth of the top-rated research, I think it is disinvesting.

I think on the civil technology side what our problem is—I want to go back to the question of hot pursuit in Federally funded research. It started under President Carter with a policy decision that led to the defunding—the defunding—of civil R&D, and it was carried on in the Reagan administration, the same mind set. So that there's very little civil R&D there. Then the hooker is that there is a white line drawn and so stated in the philosophy of OMB and the administration that the Federal Government will screech to a halt before it will carry the results of research into a cooperative mode with industry to test commercialization and feasibility and get it off the shelf and into the action.

This is a political question. And, you know, when we do this, we frustrate the yield, we deny ourselves the yield of this multi-billion-dollar Federal support in R&D, because we say that's somebody else's sphere to carry it on beyond the research findings and to the stages of proof and demonstration and eventually into commercialization, which of course is the private sector's. And that's where the articulation breaks down, in my opinion, and it's where I would see—I mean I don't go out talking to myself up and down the street about a civilian DARPA—but what I do think is that it should do exactly what Jay said, which is to keep a line of sight on where the needs are, where the opportunities, particularly in competitive or dual use technology—if you want to use another expression for it—are and where the couplings can be made.

If the Federal Laboratories, Federal multi-program Laboratories in particular, represent a very, very large public investment over many, many years and if the rules by which those laboratories live say that there is limit of 18 percent or something of your funding that you can use in cooperation with industry, I think we're out of our minds. I think that those laboratories ought to be opened up. They ought to be wheeled into position, to reinforce what industry's problem is.

Industry is very disaggregated. Wheel them into position; take those rules off the table, and let industry and those laboratories and the universities mix it up, and put the work where it can best be done. That's part of the flaws, the flaws of nonarticulation, if you will, in the arrangements we have.

I really have gone dry here, I'm sorry.

Mr. MINETA. Dr. Keyworth, did you say that we were not putting enough money into the R part of the R&D quotient and too much into D?

Mr. KEYWORTH. I did.

Mr. MINETA. Now is the development aspects of it more the industry piece of it, and so we should be doing the basic research?

Mr. KEYWORTH. I wish it were that simple. The fact is that when we talk about the concentration of defense R&D, we must realize that a very large portion of that, the heavy money end, is the testing of systems that have been built and developed over the previous five to ten years. That does not add to our new knowledge base, and that's just a simple reality. What I guess in a way I'm saying is, we—that that isn't as rich an R&D base as we think it is.

Now as we move back toward R, I think that our challenge is, on one hand, maintain a strong basic research foundation, which, by the way, I am really quite confident of right now; I think we're doing a good job. But to carry it into that applied research area where engineers are trained, where new products, and new conceptual products, develop.

This superconductivity issue we talk about—for instance—it's not a question of whether Argonne or universities are involved in it, there are thousands of people involved in it, but not one single person, that I'm aware of, is trying on one single team today in the United States—is trying—to produce an early stage prototype chip that would be the first step towards moving us into superconducting micro-electronics.

In other words, it's that place where good ideas and creative talent begin to look at practical applications. That's where our problem is. And the solution is somehow to get people in industry to talk to the people in universities and in national labs. I think we are all saying in different ways—or Bill and I certainly are saying in different ways—that maybe we don't have exactly the right way to do it, but that is the fundamental problem, and we must begin to deal with the problem.

Mr. MINETA. Thank you, Mr. Chairman.

Mr. BROWN. I'd like to request the Members to remember that we have another panel coming up after this, and we have to be out of the room by 12:30, and the very distinguished panel that is coming up later—with no intent to derogate the existing panel members—are likewise capable of speaking to these same excellent questions which the Members are bringing up.

Now, having said that, does any Member—Mr. Ritter.

Mr. RITTER. Thank you, Mr. Chairman.

I think we should have established the five-minute rule about five hours ago.

Mr. BROWN. That would have been nice, but we didn't.

Mr. RITTER. It always helps. Then we can have rounds of questioning, which I think is more interesting. It moves faster.

I just want to thank both of you gentlemen for your testimony, and, by the way, I see some real common grounds and common threads.

It seems to me that when you call, Mr. Carey, for a civilian ARPA, you are acknowledging that the present system that sets priorities is really not functional to the circumstances, not responsive.

Dr. Keyworth, when you call for a Department of Science and Technology, you are saying the same thing. I think everybody in this room understands that the system is driven by priorities of the past.

I wanted to just put some perspective on this in that there are certain things we can do in this committee, and just dealing with the science and technology problem there are certain things we can't do.

For example, the long-term versus short-term approach to capital investment in the United States, I think, is a crucial underpinning feature, and we're not the Ways and Means Committee. But I think this administration is conscious that if you are not in it for the 10-year, the 15-year, the 20-year long haul, you are not going to be in it. High-temperature superconductivity is just one of the areas, photonics, opto-electronics—just one of the areas that that relates to.

Education: we can do something about education. We're talking K through 12, through the training of engineers. We have a lot of engineering scientists training engineers to go out and be more competitive making widgets. This is something that is a long-term problem. We are not going to solve it in this committee.

Managing corporate America: We're late on the quality revolution. Those companies that have adopted it are going great guns and are doing a lot better. But let's face it, Japan has got 30 years worth of lead time on that, and that's, you know, not going to be solved.

So what we boil down to here is, what can we do as a committee of the Congress, trying to deal with the subject of, I guess, balance in the overall system and in setting priorities?

I guess if we're talking of civilian ARPA or Department of Science and Technology, what would be some of the first steps that you would envision to make this actually happen? Dr. Keyworth, do you want to lead off, and then Mr. Carey can come in.

Mr. **Keyworth**. Let me try to answer that very complicated question as simply as I can. I would say that setting up a civilian DARPA, or whatever one called it, would be enhanced greatly if we had a series of well-defined national priorities like high-definition television and like superconductivity, where we knew we had a problem and there was nobody clearly out trying to solve it in any practical way. So we have a problem identified at the beginning.

Then I think what we have got to find our way to do—and we haven't been very successful at this—is, we have got to find a way of accepting the fact that market pull actually works better than dictating the way things should happen, and so there has got to be a way to get industry to be responsible and identify what are the milestones that we should measure our own performances by.

Mr. **Ritter**. Is there enough private sector input to the current Federal R&D economy today?

Mr. **Keyworth**. Good Lord, no.

Mr. **Ritter**. I mean I ask that somewhat rhetorically.

Mr. **Keyworth**. No. I think, in fact, Mr. Congressman, if I may offer a comment here, it won't make me a lot of friends, but, you know, we have come to rely heavily upon my community, the scientific community, in setting these kinds of priorities, and the scientific community, I think, is an awful lot more interested in the health of scientists than it is either the health of science or this question of competitiveness.

We have got to find a way of bringing the leaders of corporate America, the CEO's, who are out there on the front lines, being beaten, by the way, or, either that, seeing in the long-term future that they will be beaten, and bring them in not from the perspective of seeing how much money they can save from their own R&D budgets but, instead, to get them to the tables, figure out how we could actually get out there and win in superconductivity, or get out there and win in the space program, or get out there and win in high-definition television. That we don't do.

Mr. **Ritter**. How do we bring that private sector, the veteranship of competitive wars, how do we bring that into the system as we look towards and ARPA or a Department of Science and Technology?

Mr. **Keyworth**. A simple suggestion: I think we could go back and look at how we have used commissions in the last few years and see if we can't use commissions a little bit more to guide us in implementation and a little bit less in study.

Mr. **Ritter**. I just want to point out that Dr. Sununu and his group at the White House are very serious about bringing in a greater private sector steerage to this Federal R&D economy and, I think, seeking to make personnel decisions that reflect that.

Mr. Carey, what would be the first steps you talk about?

Mr. **Carey**. I think the first step that I would talk about would be action by the President to move—without objection hopefully from the Congress—to establish a study group, or at least a planning group, to work out the structure and the roles of a civil ARPA in the executive office. I don't know that legislation would be needed; it might. When we used to have before the OSTP on a statutory basis, we had science advisors who were nonstatutory in an office, and it worked, and one of the battles that I lost years and years ago was against making it statutory. I thought that something would go out of it, out of the magic of it. So I think that this would be the first place to start.

Mr. **Ritter**. Is that free standing? Would that subsume DARPA as we know it? Would that subsume the advanced technology program?

Mr. **Carey**. I think that would be two stages. I think there would be an initial stage of kind of a working group that would be there to stake out some areas. Then I think you'd go on to that—go on from that to get, in effect, operational.

But I've just got to say that, you know, you can't just decide that from the White House you're going to lash the horses for superconductivity or high-resolution TV or something else, and then two or three things after that, without looking at the example of the Baltimore Orioles. You are not going to tell them this year to go out and win the World Series, and there's a reason you're not, and that's because there's no depth. There's no depth; there's no grass roots strength. And if you take your eyes off the sources of scientific and technical capability in this country and just lash up the horses to run up a score or to win here or to win there, you're not going to get this thing done, and that's why a civilian ARPA would deal with the articulation part of the system problem; that's what it would do. But somebody else, including the Congress, has got to worry and take care of the basic system that has to be there if we're going to be in the twenty-first century as a player and not as a loser.

Mr. **Ritter**. Mr. Chairman, in the interests of time I am going to cease, but I just wanted to mention that he has mentioned the Congress. Well, the Congress is so ultimately fragmented in this area, it's almost beyond repair. But the NAS/NAE asks to solidify the priority-setting process with some cross-cutting initiatives.

I think that's very difficult, as you point out in your testimony, but I think it's absolutely essential for this administration to go forward on that, and I think Dr. Keyworth's ideas of dealing systemically—Dr. Keyworth, I think, really sees the problem when it was necessary to address individual initiatives. I mean you had to go basically through the back door to get them all done, each and every one of them done. You got Sematech through DARPA, you got a steel initiative through the DOE; I mean you go right down the list; the system is just not set up to deal with the major challenge, which is America's competitiveness.

Mr. Chairman, thank you. I yield back.

Mr. **Brown**. The chair appreciates the gentleman's restraint.

Mr. Price.

Mr. **Price**. Thank you, Mr. Chairman.

Dr. Keyworth, you closed your statement by expressing the wish that you had been provocative, that you had perhaps provoked a question or two. And I think your remark on page 4 about the superconducting supercollider and the space station certainly falls in that category, to say that one represents a national commitment to excellence and leadership in research, the other of which is an investment in neither excellent science nor excellent technology. I suppose what I am wondering is which is which, and if you could elaborate that statement.

Mr. **Keyworth**. The superconducting supercollider is designed to explore something we don't know about yet. In other words, it's the future, and it is an area in which enormous excellence has been developed in the past, and I believe the same will happen in the future.

We put up a space station nearly 20 years ago. This space station that is—represents the core of our space program. It is something that I cannot comprehend will in any way restore our past leadership in space, will strengthen the technology that lies behind and has always lay behind our space program—and—over the eight years and before when I was outside the Government studying that question, I cannot comprehend how we can believe that the space station is either leadership or new science and technology or a way of stimulating young people. It's the past brought forward, not the future brought as close to the present.

Mr. **Price**. So, obviously, you feel that one project should move forward and the other one should not.

I'm puzzled by the context in which you bring up this example, though. You say that the main problem you are addressing is not the relative value of major projects of this sort. But it's not clear to me what the central issue is really, if it's not the necessity of evaluating such projects and establishing some kind of priority between them. So I just don't understand the way you are using that example in your statement, why it is not pertinent in some way to the central problem you are addressing. Can you help us with that?

Mr. **Keyworth**. I believe that there are immediate questions on the table in the budgetary framework that have to be dealt with. That's one of them, and there are many more, and I think what we are—probably many of us are—saying is that there is a much bigger overarching issue that has to be dealt with, which involves the way that these priorities are made, the structure in the Federal Government, the way of allocating resources. So there are two different levels of questions. We do have to decide how we are going to spend and appropriate money this year.

I think we are saying, or I am saying, wait a minute, that's not enough. We've got to look at the fact that the way we've done it over the past 20 years has left us vulnerable.

Mr. **Price**. Well, do we have any mechanism though? Those are obviously not just one-year decisions, not just short-term decisions. Do we have any mechanism for dealing with those kinds of long-range commitments, long-term priorities?

It seems to me if your—in other words, if your suggestions can't help us with something that major, then I wonder what the point is.

Mr. **Keyworth**. We are proceeding with the superconducting supercollider—

Mr. **Price**. And with the space station.

Mr. **Keyworth**. —and are proceeding with the space station, and we are proceeding with the human genome project and lots of other projects. So those decisions are being made, and I think the point that I've at least tried to make is that the sum of all those together is going to leave us using our resources inadequately to deal with the kind of competitive threat that we are faced with from the outside.

Mr. **Price**. All right. I know our time is limited. I'll leave it at that.

Let me turn quickly to Mr. Carey for one question.

You indicate in your statement that you're not really talking about the quantity of spending but that you do have some serious questions about balance. But it seems to me then you revert immediately to questions of quantity because you talk, I think, quite justifiably about the declining array of funding of basic research proposals, wherein about only 22 percent of the high-value proposals can be supported with money available. Believe me, we all hear about that all the time. I especially do in my district with as many university-based researchers as we have.

You say, however, that's not a funding problem, not a problem of overall commitment, it's a problem of balance. Again, can you elaborate? What is the imbalance precisely that you are pointing to?

Mr. **Carey**. I think the imbalance is the one that Jay Keyworth alluded to—too much money on development and a fractured jurisdictional situation which funds development in the budget and in the Government on one line of sight while another jurisdiction, in the Congress and in OMB, will set a target limit, if you will, a ceiling, on biomedical research support or training or whatever. This is how we get into trouble and how we settle and solidify claims on the budget year after year.

If we ante up the development side of the budget as we have been doing, particularly in the national security area for a number of years, the amount of support that is provided in that year's budget has to be increased substantially in every out year after that, or you find yourself under a Gramm-Rudman situation in a position where you don't know what in the hell—excuse me—to do, because all the funding requirements and claims that you have built into your base are coming due.

Those bills for R&D have to be paid, and then you start backing off, and it's just an incoherent way to do it. And the imbalance that I am talking about is, I think, a reflection on the administration's and the Congress's reluctance, for example, to invest to see research funds as investment and investment in education as a systems field, if you will, and we don't look at it that way. And I think that we need more time as much as anything else, not just wisdom but time, to think these things out. And if we don't go to a multiyear budgeting appropriations approach for something that is not created to respect fiscal years, then we're still going to keep it in an incoherent posture. And I hope that—really—the Congress will look on a multi-year funding proposal not as something that science is trying to swipe out of the safe but, really, as a much better process on which to carry out national policy.

Mr. PRICE. Do you have anything to add on that particular point, Dr. Keyworth? Do you find the statistics equally alarming as regards our basic science commitment? And I suppose also the biases of the system that Mr. Carey has suggested damage basic research more than they do the development budget.

Mr. KEYWORTH. I reflect exactly the same concerns about basic research in the long run. I must say that I would love to see a two-year funding cycle, but I think we're going to need something bigger and bolder than that, and that is some means to do strategic thinking about our long-range use of science and technology in the Federal Government.

Mr. PRICE. Thank you, Mr. Chairman.

Mr. BROWN. Thank you, Mr. Price.

Mr. Walker, we've been waiting to hear from you.

Mr. WALKER. Thank you, Mr. Chairman. I will try to be brief.

First of all, Dr. Keyworth, I would just say on the question of the space station, I think that there is some desirability in putting infrastructure in space that allows us to do good science. I mean I see the space station like building a Berkeley Laboratory or like other facilities that we build here on earth in order to do good science. You've got to have some infrastructure in space in the first place, and to say that the station itself is not the highest-tech way to go, well, you know, somewhere along the line you have got to build some kind of capacity in order to do things for the future.

But I really wanted to deal with the question of the Department of Science and Technology that you raised. Would you include space as a part of that department?

Mr. KEYWORTH. Space?

Mr. WALKER. Yes.

Mr. KEYWORTH. I certainly would.

Mr. WALKER. You'd put NASA into the department?

Mr. KEYWORTH. Yes, I would.

Mr. **Walker**. Have you thought through just exactly what the structural components of that department would be? I mean do you have something prepared where you have looked at the structure?

Mr. **Keyworth**. I, and, before, my staff in the White House—we thought through this for quite a few years, and I have ideas to suggest. I think it is an issue that has got to be thought through very carefully, and I think, as Bill Carey has pointed out, we must remember the fiasco of the Department of Energy shows how poorly you can do it if you don't have clearly in mind what your goals are.

Mr. **Walker**. Yes. I was intimately involved in that fiasco over in the Government Operations Committee, and I regard it as one of the worse votes I have ever cast in the Congress and am sorry that I participated in the process that created that.

On the other hand, you know, I have come to believe that there is some merit in looking at some kind of structural entity that would allow at least somebody to sit at the Cabinet table that addresses the future on a day-to-day basis. So I think that there is some desirability in looking at that and, in fact, put together a bill in the last Congress, which I never submitted and so on because I want to talk it through.

What I'd be interested in is maybe having you review that legislation, take a look at it. Among other things, it eliminates the Department of Energy and takes a lot of the R&D that is now there, puts that under the Science and Technology Department, and then spins off the weapons programs over to Defense, and so on.

But it is an attempt to deal with some of those issues, and since you've raised the issue I would appreciate the opportunity, if I could, to have you review it from your perspective and see whether or not there is something there worth pursuing.

Mr. **Keyworth**. I would very much like to do that.

Mr. **Walker**. Okay. Good.

Mr. **Carey**. If I might just make a comment, Mr. Walker.

Mr. **Walker**. Surely, Mr. Carey.

Mr. **Carey**. I heard you say that it would be fine to have somebody at the Cabinet table whose mind is on the future. I assume that you have read a book called *The Thirteenth Man* by the—

Mr. **Walker**. I have not read it.

Mr. **Carey**. It would be very informative. Here was a Secretary—first Secretary of Education—who came and took the thirteenth seat and had no luck at all in illuminating the deliberations around the Cabinet. It just doesn't automatically follow, is simply the point that I am trying to make, and to put the whole idea on what happens out of that chair of inspiration at the Cabinet table just—I don't think is the way to do it.

Mr. **Walker**. I think you make a legitimate point. On the other hand, I know also it is not going to happen if no one is there. I guess I come to the conclusion that somebody is better than nobody in a number of these instances, and the fact is that as we see the whole pattern of deliberation has gone on, science and technology has often taken the back seat to other higher priorities that get defined, and then somebody comes in and says, "Oh, and, by the way, we need to do something about science and technology."

No one sees how you coordinate these things so that you end up with science and technology being considered as a matter of the totality of national policy. Somewhere there needs to be a voice like that, and I don't see that that voice is taking place in Cabinet deliberations at this point.

Thank you, Mr. Chairman.

Mr. BROWN. Thank you very much, Mr. Walker. You raised a question that I was going to ask, myself, if I had more time, but I appreciate your raising it.

You are going to give us the answer in writing, aren't you, Dr. Keyworth?

Mr. KEYWORTH. Yes.

Mr. BROWN. Okay.

Thank you very much, gentlemen. You have been very helpful, and of course, as you recognize, the Members are deeply interested in your contribution.

Mr. CAREY. Nice to see you again, Mr. Chairman.

Mr. BROWN. Thank you.

Mr. KEYWORTH. Thank you.

Mr. BROWN. We now call our second panel: Dr. Mary Good, Dr. William Brinkman, Dr. John Waymouth, and Mr. Laurence Hecht.

We very much appreciate your willingness to be here with us this morning and to sit through the previous panel and listen to the sense of what the Members have on their minds.

Dr. Good, would you like to proceed.

STATEMENTS OF DR. MARY GOOD, PRESIDENT, ENGINEERED MATERIALS RESEARCH, ALLIED-SIGNAL CORPORATION, MORRISTOWN, NEW JERSEY; WILLIAM F. BRINKMAN, EXECUTIVE DIRECTOR FOR RESEARCH, PHYSICS DIVISION, AT&T BELL LABS, MURRAY HILL, NEW JERSEY; JOHN WAYMOUTH, FORMER DIRECTOR OF R&D LABORATORIES, GTE LIGHTING PRODUCTS GROUP, DANVERS, MASSACHUSETTS; AND LAURENCE W. HECHT, EXECUTIVE DIRECTOR, IACOCCA INSTITUTE, LEHIGH UNIVERSITY, BETHLEHEM, PENNSYLVANIA

Ms. GOOD. Thank you very much, Congressman Brown and other Members who are here. I very much—I have a very brief statement, and that's what I have, and then we'd answer any questions if you like.

Mr. BROWN. All right. I think it would be best if we presented each of the statements first, and then we'll ask questions of the panel.

Ms. GOOD. Fine.

I very much appreciate the opportunity to discuss some fundamental issues of U.S. policy in science and technology with you today. At just the time when we are experiencing difficult budget decisions, particularly the need to reduce our national deficit, we find ourselves in an ever increasing competitive climate of global trade.

Our success is significantly dependent on our ability to maintain leadership positions in industrial development tied directly to our science and technology base. At the same time, we are facing an educational crisis where the public literacy and technical capability is not adequate to maintain the technologically based infrastructure of the twenty-first century. The science and math scores of our children are alarmingly poor, and the number of U.S. citizens choosing to pursue scientific and engineering careers is steadily decreasing. Thus, there is probably no more important agenda today than the strategic positioning of our science and technology efforts, and the priority issues, as I see them, are the following:

The first one is, how do we effectively support the R&D necessary for our competitiveness efforts? The second, what is needed to provide the technology base for our continued military superiority? Third, how do we provide the research and development necessary to continue our programs in health and environmental improvement? And, fourth, can we strengthen the science and technology infrastructure in our schools, colleges, and universities so that the science base for the next generation will be available as needed, including adequate scientists and engineers available to maintain our leadership positions?

Now the issues above have not been prioritized amongst those four, although I think it should be clear that if we cannot assure our overall economic vitality, the other issues are moot, because we could not afford them. My position today is that all four of these goals must be met. If we do not meet them, the quality of our lives and that of our children and grandchildren will be greatly diminished. Thus, the question before us is not whether we meet all of these goals, but, rather, how do we meet the goals in the most cost-effective manner.

Now since you have already heard from several witnesses, both this morning and yesterday, who have laid out the problems, I would like to focus my limited time on some of the practical questions raised by the hearing charter document. The major issues are, how do you identify effective mechanisms for evaluating the adequacy of funding support for science and technology, for determining promising directions for new research, and for establishing priorities in the Federal R&D budget? And, truly, that is the issue. An equally important question, however, is, are we getting our money's worth in the programs we fund?

An important lesson has been learned over the past few years by the American industry. To be competitive, we must be more productive and we must produce a higher quality product at a lower cost. The Federal Government can do no less. It cannot be competitive as a government unless its programs are effective and efficient. Increased productivity is the key everywhere, and that's in Government programs as in the rest of the world.

Now let me speak to the evaluation mechanisms for each of the areas I outlined earlier. Since the concerns include commercial interests, defense activities, human welfare questions, and our future manpower needs, it is obvious that strategic planning will require a cross-cutting evaluation where mission-oriented programs are optimized to positively impact the other areas of need. In other words, we need to get these things integrated so that each of these programs play off of each other.

Presently, the best group to convene and coordinate a strategic planning group is the Office of Science and Technology Policy. The President's intent to make the head of OSTP the science advisor, a special assistant to the President, is welcome. This move, coupled with a high quality President's advisory council for technology, should provide OSTP with the clout necessary to actually manage a national strategic plan for science and technology.

Early in the budget cycle, the Science Advisor should convene a planning group that includes the new Undersecretary for Science and Technology at Commerce, the Undersecretary for Research and Development at Defense, the appropriate Undersecretary at Energy, and the Directors of NASA, NIH, and NSF. At this level, the group could construct a framework for fundamental research focused on universities—because all of them do that—they could assess the role of the national laboratories to each of the missions—because each of them have some interests in those—and they could coordinate major technology programs so that the industrial spin-offs are optimized to the extent compatible with the mission objectives.

I am convinced that one major contribution, or one major contributor to our vitality in science and technology has been our diversity of objectives. However, I believe that diversity can be maintained within the framework of a coordinated effort. Our total Federal R&D budget is quite large. With appropriate cross-cutting cooperation and the determination of the executive and congressional branch to sunset proper programs promptly—and may I emphasize that that's as important as starting new ones, is to know when to stop doing ones that are no longer productive—I believe we can afford the resources necessary to maintain our leadership in science and technology, including our share of the so-called big science initiatives.

Now before concluding, I would like to address a few specific ideas which I believe are crucial to the overall program and overall progress. The first one is international cooperation and sharing of large science projects. I believe the time has come where some of the other industrialized nations should carry a significant fraction of the resource burden for large science projects carried out for advancing the scientific frontier and for major global environmental and climate studies.

NASA, the Department of Energy, NIH, and NSF should develop a strategic plan for having significant European and Japanese participation, both funding and personnel—we get personnel from time to time, we don't get funding generally—in several of the planned "big science" facilities, in global environmental research, and in the human factors research. The NSF drill ship program for global geology is an excellent model. The SSC, the global mapping for the greenhouse effect, and some astronomy centers are excellent immediate candidates.

Secondly, joint agency funding. One thing I would hope would come out of the cross-cutting strategic planning would be specific joint agency funding in certain areas. Current candidates include materials research centers, supercomputer centers, science and technology centers, fellowship programs, and the list goes on. Many of these activities have dual potential for defense and the civilian agencies. Why not leverage the defense R&D budget by optimizing its effect in the civilian section to the extent consistent with defense needs? The same argument holds for joint projects, programs between Energy and NSF, between NIH and NSF, and all other various combinations.

And, three, science and education and public understanding of technology. The national need for improved science and math education and for the public to understand the technological environment is so great that all of the agencies cited above must contribute to the solution. Every Federal R&D project should have an educational component and a pre-planned public relations strategy.

One of our problems is, our public does not understand what we are trying to do, and they don't have any good handle on what that's all about. So it seems to me the programs ought to have that as a requirement; a piece of the program should be simply built in.

In conclusion, I believe that the prioritization of science and technology programs would occur naturally through the give-and-take of the cross-cutting strategic planning outlined above. The final Federal R&D budget will still have the individual mission components, but an overriding plan would be available for the Congress to evaluate for balance and for national interests, and if you required them to provide a five-year strategic plan, it would also allow you to have some information as to what their long-term thinking is all about, and then appropriations could then be made with the priorities in place.

Thank you, sir.

[The biographical sketch of Dr. Good follows:]

BIOGRAPHICAL INFORMATION

Dr. Mary Lowe Good
Senior Vice President-Technology
Allied-Signal Inc.
P.O. Box 1021R
Morristown, NJ 07960-1021

Academic Background:

- B.S., Chemistry, University of Central Arkansas, 1950
- M.S., Chemistry, University of Arkansas, Fayetteville, 1953
- Ph.D., Chemistry, University of Arkansas, Fayetteville, 1955

Former Professional Occupations:

- President-Engineered Materials Research, Allied-Signal Inc., 1986-88.
- President-Director of Research, Signal Research Center Inc., (now Allied-Signal Engineered Materials Research Center) 1985-present.
- Vice President-Director of Research, Corporate Research Center, UOP Inc., 1980-1985.
- Boyd Professor of Materials Science, Division of Engineering Research, Louisiana State University-Baton Rouge, 1978-80.
- Boyd Professor of Chemistry, University of New Orleans, 1974-78.
- Professor of Chemistry, University of New Orleans, 1963-74.
- Associate Professor of Chemistry, Louisiana State University-New Orleans, 1958-63.
- Instructor and Assistant Professor of Chemistry, Louisiana State University-Baton Rouge, 1954-58.

Membership in Professional Organizations:

- American Chemical Society, 1951-present; President 1987; Board of Directors, 1971-80; Board Chairman, 1978, 1980.
- International Union of Pure and Applied Chemistry (IUPAC), President, Inorganic Division, 1980-85; Member of Bureau, 1985-89.
- National Science Board, 1980-present. Chairman, 1988; Vice Chairman, 1984.
- Royal Society of Chemistry (Brit.), 1960-present.
- American Institute of Chemists, 1965-present.
- Sigma Xi, 1952-present.
- Alpha Chi Sigma, 1975-present.
- Research Directors of Chicago Association, 1981-present.
- Catalysis Club of Chicago, 1981-present.

Other Professional Activities:

- National Research Council/National Academy of Science Panel on the Impact of National Security Controls on International Technology Transfer.
- National Research Council Commission on Physical Sciences, Mathematics, and Resources.
- Board of Directors, Industrial Research Institute, 1981-present.
- Board of Directors, Oak Ridge Associated Universities, 1971-76.
- Board of Trustees, Rensselaer Polytechnic Institute, 1982-present.
- Board of Directors of the National Institute for Petroleum and Energy Research (Bartlesville, OK).

- National Bureau of Standards Panel for Materials Science, 1980-86.
- Advisory Panels, NSF Chemistry Section, 1972-76; NIH Committee on Medicinal Chemistry, 1972-76; Office of Air Force Research, 1974-78; Chemistry Division of Brookhaven and Oak Ridge National Laboratories, 1973-83; Mayor Burne's Chicago Task Force on High Technology Development.
- PACE Review Panel, Universities Space Research Association.
- Advisory Panel, Chemical Technology Division of ORNL
- Advisory Panel, Catalysis Program, Lawrence-Berkeley Laboratory.
- External Advisory Committee, College of Engineering at Louisiana State University.
- Review Committees for Chemistry Department Graduate Programs at University of Texas-Austin, University of Texas-Dallas, University of California-San Diego, and Harvard University.

Memberships in Community Organizations:

- Zonta International, 1965-present. Chairman, Amelia Earhart Awards Committee, 1978-present.
- Chicago Network, 1983-present.

Published Works:

- Approximately 100 articles in refereed technical journals.
- Several publications on science policy and research management strategy.
- One book: Integrated Laboratory Sequence: Volume III - Separations and Analysis, Barnes and Noble, Inc., New York, NY, 1970.

Honors:

- Delmer S. Fahrney Medal, Franklin Institute, 1988
- Elected Member, National Academy of Engineering, 1987.
- Elected Fellow, American Association for the Advancement of Science, 1986.
- Gold Medal, American Institute of Chemists, 1983.
- Scientist of the Year, Industrial Research & Development Magazine, 1982.
- Garvan Medal, American Chemical Society, 1973.
- Florida Award, Florida Section, American Chemical Society, 1979.
- Herty Medal, Georgia Section, American Chemical Society, 1975.
- American Institute of Chemists Honor Scroll, Louisiana Chapter, 1974.
- Distinguished Alumnae Citation, University of Arkansas, 1973.
- Agnes Faye Morgan Research Award, 1969.
- Honorary Doctorates: University of Arkansas, 1979; University of Illinois-Chicago, 1983; Clarkson University, 1984; Eastern Michigan University, 1986; Duke University, 1987; St. Mary's College, 1987; Kenyon College, 1988.
- Honor Societies: Iota Sigma Pi, Phi Beta Kappa.

Mr. **Brown**. That's a very impressive statement, Dr. Good. I'm going to offer you the job of Science Advisor to the President right away and see what happens.

Mr. **Ritter**. We should make that bipartisan, Mr. Chairman.

Mr. **Brown**. All right.

You're next, Dr. Brinkman.

Dr. **Brinkman**. Thank you.

I will try to summarize my statement rather than read it.

I'd like to address very quickly four issues: one, the role of the Federal Government in civilian research and development, the Federal priorities in science and technology, and, finally, concerns regarding basic research.

We've heard a lot of discussion this morning on competitiveness, and the point I would like to make about competitiveness is to try to bring in the fact that the United States has not suffered in basic science and in what I would call applied research—namely, invention of new technologies—where we have suffered in development of products, in the product realization process. That's a process that has been most difficult for us to do effectively in the last ten years. And the question is—the important point about that is, that is primarily a civilian and industrial responsibility.

So when you look at the situation today, the Federal Government's role in that must be as a supporter of industrial research, not a funder of industrial research but, rather, a supporter, and it has to do it in such a way to greatly increase industrial research. If we look at any NSF science indicators, for example, it's very clear that industrial research in this country is small compared to our GNP. We must increase that level, and the way we can do that is by the Federal Government doing such things as supporting tax credit for R&D and other stimulating economic moves that would help the research. And I think that is difficult; that is development.

Jay Keyworth was talking about development, saying we did too much of it. What he meant was, we were doing too much development on the defense side, not on the civilian side. We are doing too little development on the civilian side. And there's a very big distinction there, and it's important to recognize that. The cross-talk between those two development activities is very small, and, in fact, the cultures are so different that, in fact, they can be detrimental to each other and not enhance each other at all.

Now I don't mean to imply that the Federal Government's only role in civilian R&D is to stimulate industrial research and development, but it obviously can also help with stimulating universities. University research plays a very, very important role in training people, and the Federal Government is still the major funding source for that. So we need to continue to see to it that that's healthy. I think in that area the universities can do a lot more and Government could do more to help emphasize manufacturing science and subjects relevant to the product realization process.

I also believe that the national laboratories could do more in the applied research area directed toward civilian research. I was at Sandia for several years, and I can list a number of things that we did there. We had down hole seismic generators for geological structure examination, we had strain measurement apparatus for looking for fault structure around oil wells, we invented batteries, we invented some compact computers[4] and electronic devices, various things that were relevant to the private sector.

My point about the national laboratories is that the national laboratories cannot contribute successfully if it isn't part of their mission to do civilian applied research, and one of the things I found most distressing is, I was being appropriated money to do a specific job and I was being told at the same time by Congress that I should use some fraction of it to do technology transfer. I think that's the contradiction in telling me what to do—the paradox in telling me what to do.

In your letter, you asked us to test how civilian technology might be identified for high potential for national payoffs, and when I thought about that, that particular question, it seemed to me that that has not been our problem. We have no problem whatsoever identifying high-technology areas. We have been looking at biotechnology, look at neuro networks, look at superconductivity, you take your choice; we identify these things with no problem whatsoever, and we even, in fact, lead the world in research often in these things. I come back again that our national dilemma is the product realization process and how to stimulate that in industry today, and we need to work on that and assure our economic health in the future.

The next thing I wanted to address quickly is the question of priorities, and much discussion has been given to that today. I essentially agree with Jay Keyworth in a very simple fashion. I believe that if you want somebody to set priorities you need to give them the authority—give that person the authority to do so, and one of the objections I have to our current structure—namely, the Office of the Science Advisor—is that the science advisor really does not have explicit authority, and I think that some kind of budget—if you want to stick with the science advisor structure, some kind of budget concurrence of some sort so that he has some real power invested in him is absolutely essential. I do not see how we can do that in any other way.

As for other suggestions as to how to work on priorities, I would suggest that maybe instead of looking at something like the SSC versus genome project, we look at the fields of science, particularly in basic research, where you can accumulate these kinds of facts, look at the fields of science and try to understand how you are supporting this field, what its relevance is to the overall goals of this country, goals which have been defined in terms of intellectual contribution, technology development, et cetera, and then you can make a better judgment about the total impact of that field, and perhaps you could force the sponsors of research in that field to set their own priorities more effectively.

I likewise believe that this Congress and the Executive Branch could force the priority setting down[5] if they were to be able to agree to some long-term guidelines on large budget items, such as the 240 line. That would, again, force the priority setting down into the lower levels of management of the Federal Government.

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[4] Dr. Brinkman indicated "some compact computers" should be changed to "a compact computer" to accurately reflect his intended meaning.

[5] Dr. Brinkman indicated that "to lower levels" should be added after "down" to reflect his intended meaning.

Finally, I would like to say a few words about the National Science Foundation. The National Science Foundation is the one place where basic scientific research—that is, research directed at trying to understand nature better—is encouraged and pursued without any other questions asked, such as relevance to technology and industrial competitiveness, and I think that has been its tradition, it has been the source—the final source for funding of basic research.

Today, what I see more and more is the science budget being—NSF—as being justified on the basis of competitiveness. I think that is a dangerous direction to be going, and I find that on that basis what happens, many of the sub-fields of science, even the fields I am very familiar with—condensed matter physics, material science—are hardly getting any increases at all in the National Science Foundation, and other things are being funded. These areas—materials science and condensed matter physics—are areas which have a tremendous relevance to the whole semiconductor industry and other parts of American industry. I believe it's essential that a National Science Foundation be the patron of basic science in this country, and that has to be its objective and goals, and no other objectives.

In summary then, I think that the central role of the Federal Government in competitiveness is to create a favorable climate for industrial research and development, and particularly development. I think priorities can be established in science and technology if someone has the authority to do so, and, finally, I think the role of the National Science Foundation's primary function must be to be the patron of research.

Thank you.

[The prepared statement of Dr. Brinkman follows:]

Statement of

W. F. Brinkman
Executive Director, Research, Physics
AT&T Bell Laboratories

before the

Subcommittee on Research, Technology and Science
of the
Committee on Science, Space and Technology

March 1, 1989

Chairman Walgren, members and staff of the Subcommittee on Research, Technology and Science - thank you for the opportunity to express my views on the American science and technology effort. I would like to briefly address four issues. The question of competitiveness, the role of the Federal government in civilian research and development, federal priorities in science and technology, and finally my concerns regarding basic scientific research.

Today, much discussion of science and technology centers on competitiveness or economic health as measured by our balance of trade. Indeed, more and more S&T programs are justified on this basis. Yet, any sensible analysis indicates that it is not our lack of science and technology that is our weakness. Our major problem is in the development of products and the larger product realization process. This process has many aspects to it: concentrated flexible development, efficient manufacture, effective distribution and marketing, and sales. The development work that is part of our total R&D effort involved in product realization is necessarily a function that belongs to private industry. Although R&D is active in the U.S., especially in some telecommunication areas, NSF surveys of overall R&D as a percent of the GNP, casts doubt on the overall adequacy of our civilian R&D. It is not the only part of the product realization process that needs improvement. Some industries recognize this and many of the quality campaigns in American companies are aimed at creating an effective product realization process. But civilian R&D is the focus of our attention here and in my view, the role of the Federal government must be to encourage increased expenditure on R&D in private industry. Thus, for example, making the tax credit for R&D expenditures permanent would help. The Federal government must also work toward creating a financial climate where industry is inclined to place emphasis on long-term growth rather than short-term profits. (It can also do a lot to create a change in the American way of thinking with respect to reaching out to understand foreign cultures and economies.) One should not be deceived by our large R&D expenditures on defense. While it may create technology spinoff, it also creates a culture not oriented toward commercial product realization. Its net contribution to civilian R&D is small.

Thus, the primary role of the Federal government in civilian R&D is that of shaping an economic environment for industrial R&D.

This, however, is not the only role. The government can sponsor and does applied research, i.e., research where the goal is to invent or create new technology that is relevant for the civilian sector. Research in materials processing, laser technology, new electronic and optical devices, for example, helps to establish a technology base which feeds into industrial applied resarch and creates the knowledge base from which specific products are developed. The role of universities here is important not just for the knowledge base generated by their research, but also because of the training of scientists and engineers. This central role of university research, supplying well-trained scientists and engineers, can only remain healthy through adequate funding at the federal level.

The importance of training our youths to meet the industrial challenges of the future cannot be overemphasized. A central part of this challenge is training in manufacturing sciences. Universities must help in formulating and teaching modern manufacturing techniques that lead to a balanced approach to the issues of automation and human effectiveness.

Having served as Vice President of Research at one of the national laboratories, I believe they can play a larger role in what I have labeled applied research. While at Sandia National Laboratories, which as you know has been managed by AT&T since 1947, we had a number of inventions that were transferred to the private sector. Some in the energy field, such as a down hole seismic generator for measuring geological structures, and a strain measurement apparatus for understanding fault structures important in oil drilling. Other inventions were in batteries, compact computers, and electronic devices. Because Sandia is a nuclear weapons R&D laboratory, it is perhaps better oriented toward technology than other national laboratories, but the multipurpose national labs could be more oriented toward civilian technology and technology transfer. To be effective, however, such orientation must be considered an integral part of their mission. They cannot be told to spend some fraction of the budget on technology transfer when that budget has been appropriated to them for other purposes.

In the letter of invitation, it was asked how civilian technologies might be identified that have high potential for large national payoffs. It is my view that this is not our problem as a nation. We move very quickly into new areas of research and technology. Witness the effort we have created in biotechnology, in neural networks, or most recently in superconductivity. Our national dilemma is in the product realization process, particularly when the products require a complex mix of technologies in an evolving marketplace where long-term support is essential to an agile response to the changing market trends. There is no better example of this than the semiconductor industry where product evolution is demanding ever more sophisticated processing and design requiring long-term commitments to research and development. This industry is essential to future economic breadth, yet it is finding it difficult to survive.

The budget deficits over the past years have led to a tightening of federal outlays for science and technology. This naturally brings up the question of priorities, particularly given the disparate views of Congress and the previous administration regarding budget outlays. The scientific community is also greatly concerned because many proposed activities are being kept alive by marginal levels of funding that will sustain the effort for another year. This leads to a situation where projects either never get finished, or are done in such a way that they are completed late with reduced goals so that their net impact is marginal. In physics we have for some time been looking at a situation in which our requests, which include the Superconducting Super Collider, the Relativistic Heavy Ion Collider, and the Advanced Photon Source, to name a few, clearly cannot be carried out without essentially doubling the physics budget. The budget itself has, of course, hardly changed relative to inflation. It seems clear that priorities need to be established. But how?

I have a few suggestions. I think it is difficult for Congress or anyone to compare the SSC with the human genome problem. It is easier and perhaps more sensible to look at the total funding in fields of science and technology and try to decide whether these fields are adequately supported relative to one another. Then the people funding these fields would be forced to set their priorities. Thus, one would compare the support of physics versus the support of molecular biology rather than comparing the SSC with the human genome project. This suggestion is meant to apply primarily to the sciences where reasonable definitions of the boundries between fields can be established. Another suggestion is for the Congress and the Executive Branch to agree on a set of general guidelines, or even numbers on certain budget segments, the 240 line, for example, for the next several years, perhaps five, leaving the detailed budget to be determined each year. A realistic guideline would force realism upon lower levels of the government, and hard choices would have to be made.

There has been much discussion of the role of the science advisor and the Office of Science and Technology Policy (OSTP) in the prioritization process. Currently, the science advisor advises the President and does not have any formal approval role for the science and technology parts of the budget. His power, therefore, largely depends on his relationship with the President. I am convinced that the science and technology policy of the United States should not be so strongly dependent on the rapport of one person with respect to the president. It is clear that the OSTP and the science advisor cannot have a strong influence over budget priorities without some form of budget concurrence. There are two possible solutions to this problem. One is to explicitly invest budget approval in the OSTP at or just below the OMB level for science and technology programs. The other is to create a Department of Science and Technology which would contain such agencies as the NSF, NASA and the energy part of the DOE. Since the latter is a much larger change requiring legislation, the former is surely easier to execute and would be worth trying. Given explicit budget approval at the OSTP, it would surely need a strong scientific advisory group.

Finally, the last point of concern I have which has more to do with my views as a scientist than as a representative of industry, is the preservation of basic research in a federal process that is placing increasing emphasis on issues such as competitiveness. The increase in the National Science Foundation budget is a strong statement by the President and Congress of the importance you place on science and technology. We, as scientists, can only applaud. However, it is easy to allow the urgency of economic competitiveness in the near term to override the importance of long-term relatively undirected basic research. Indeed, today most of the increases in the NSF budget are channeled into programs directed explicitly at competitiveness issues. Even materials science, with its obvious relevance to high technology industries such as the semiconductor industry, has not gotten any substantial increases. Indeed, Solid State or Condensed Matter Physics has not nearly kept up with inflation. This is at a time when it is recognized that we are not training enough solid-state physicists who do experimental work! The National Science Foundation must be the patron of basic science in this country, and in basic science a bottoms-up approach to management is essential.

In summary, industrial development is the central R&D issue relevant to competitiveness and the primary role of the Federal government is to create a favorable climate for it. Priorities can be established in science and technology if someone has the authority to do so. And, finally, the National Science Foundation's primary function must be to be the patron of basic research.

Thank you for allowing me this opportunity to testify before you.

Mr. **Brown**. Thank you very much, Dr. Brinkman.

Dr. Waymouth.

Dr. **Waymouth**. Mr. Chairman, honorable Members, staff of the subcommittee, ladies and gentlemen, thank you for offering me the opportunity to present my views on this most important subject at this hearing.

I'd like to preface my remarks with the statement that I speak only for myself and that my views do not necessarily represent those of my former employer nor of any other group of which I may happen to be a member, and any remarks I might make about industrial and academic science and technology are to be taken as general opinions and are not to be construed as applying to any specific persons or organizations.

In my oral presentation, I'd like to address two major topic areas. First, my opinion that total U.S. spending for research is adequate, in fact, that the U.S. leads the world, but in common with several other speakers, I believe the U.S. derives little or no benefit from its preeminence in research because the coupling of research results into the product and process development and improvement activity is very poor; and the second topic I wish to discuss is my view of factors which should be considered in attempting to choose among competing programs or projects in a situation of constrained funding.

First, in my opinion, insufficient research is not the cause of our present economic woes. Product and process development and improvement is where we are being beaten. In many bread-and-butter manufacturing industries, I believe we're being outmanned two or three to one in the PPDI function by Japan and other competitors.

Furthermore, it is my opinion that in the vast majority of American industry the PPDI function is very poorly coupled to the scientific community, especially the academic science community. PPDI workers do not have time to attend scientific conferences. In many cases, they don't even have time to read.

The academic science community, especially the physicists, look with scorn at product and process development and improvement and regards the people who do it as little more than technicians. For their part, the PPDI workers tend to look on academic scientists as ivory tower eggheads.

For a whole spectrum of reasons, therefore, ranging between overwork to culture clash, there are great obstacles to bringing the results of scientific research into application, and the most active and productive channel is through the efforts of salesmen trying to sell materials and equipment which, more often than not, are foreign.

It is my observation and opinion that by the time new research results reach the point of use in U.S. industry they are already widely known throughout the world and U.S. industry has lost whatever advantage it might have gained in the preeminence of the U.S. in research.

Now the establishment of the NSF engineering centers and technology centers in universities is certainly a step in the right direction to bring the needs of industry to the attention of academic researchers. However, the degree to which these will facilitate bringing research results down to the point of use remains, in my mind, an open question.

I believe that what is needed is an active partnership between academic scientists in process and product development and improvement—scientists, engineers, and technicians—and this will require and promote the softening of some deeply held, ingrained attitudes.

In my written statement, I suggest that as a mechanism, the requirement of industrial partnership—and partnership—as a condition of funding of academic research projects. The Technology Transfer Act of 1986 mandating such partnerships for Government laboratories is a possible model.

In his example of the possible gains to be achieved, I would like to concur with Dr. Keyworth in citing the unparalleled burst not only of technical but also of scientific productivity that resulted from hands-on participation in nitty-gritty development by academic scientists during World War II.

To turn to my second major topic—how can the choice rationally be made among scientific and technical projects competing for the same funds—the three Weinberg criteria have been cited in the charter for this hearing. Now I believe that these three are merely different aspects of one; to-wit: what is the value of success?

Now there are, in my opinion, other aspects to this criterion than those embodied in the Weinberg statements, and from my perspective the commercial utility of success is an intrinsic part of its value. Although it is difficult to predict ultimately utility in advance, a broad generalization is possible, in my opinion. Those scientific investigations which deal with what happens on earth are infinitely more likely to produce results useful on earth than those which do not. Faced with the necessity of choosing only one among the SSC, the space station, and the sequencing of the human genome, I would unhesitatingly pick the latter.

To the question, what is the value of success? I would add two more: What is the probability of success? and, what is the anticipated cost of success, and when is it expected?

Two points regarding the probability: First, it's inversely related to value. The higher the value, the riskier the project. All the easy things have already been done. But a research and development portfolio must include its share of high-risk, high-value projects. Otherwise, we only hit for average, and we never hit a home run.

The second point: Engineers and scientists are incurable optimists. They typically overestimate the probability of success by a factor as much as two. In my experience as an R&D director, I evolved a rule of thumb with respect to cost and time that I found useful if for no other reason than to warn me against overoptimism.

First, to improve and develop a newly discovered phenomenon or technology in a major figure of merit by one factor of ten beyond that apparent at its first discovery takes between 10 and 100 person years, 1 to 10 million 1988 dollars. Second, each additional factor of 10 improvement costs 10 times as much as the previous one. So this scales rather quickly, say tenfold, $5 million; 100 times, $50 million; 1,000 times, $500 million; and so forth.

The time for each level of improvement is, of course, simply the corresponding number of person years divided by the personnel that can be made available.

Now an example of this type of scaling is magnetically confined thermo-nuclear fusion, which enjoyed a proof of principle at a cost of a few million dollars in the late 1950's with a figure of merit product of plasma density times confinement time one one-millionth of what was needed for a self-sustaining reaction. A quarter century and many billions of dollars later, that figure of merit has been improved by a factor of 100,000, but there is still one more factor of 10 left to go. Inertial confinement fusion, I think, faces similar scaling problems.

Now this rule of thumb provided me with a guideline as an R&D director for deciding when not to fund even the low-cost early stages of a project. Unless I could foresee to be available the ultimate resources required for the number of factors of 10 improvement needed to make a development a commercial reality, I didn't even waste money starting it because I knew I wouldn't have the money to finish it. And although the resources available to the Nation permit tackling projects with many more factors of 10 improvement required than those my former labs could deal with, the principle remains the same. We should even at the national level ask what the ultimate cost is likely to be before we start wasting money on a job we can't finish.

Now to summarize both points, I believe that our economic competitors do a better job than we do of coupling the fruits of research into the product and process development and improvement. I believe that the U.S. PPDI effort is both insufficient in size and inefficient in performance as a result of its poor coupling to science.

Now the ability of Congress to change this situation is not clear in my mind, except perhaps in the form of gentle nudges, such as changed rules governing research contracts and possibly in the conduct of business, but unless these deficiencies are corrected, our world-wide competitors are much more likely than the U.S. to be beneficiaries of our improved prioritizing and focusing of research; and, finally, I differ strongly from the vast majority of my scientific colleagues in believing that the ultimate utility of the results of research must figure heavily in its anticipated value of success, which in turn is one of the major criteria for establishing priorities for public funding of research.

Thank you.

[The prepared statement of Dr. Waymouth follows:]

STATEMENT
OF
DR. JOHN F. WAYMOUTH

(Former Director, R&D Laboratories, GTE Lighting Group: Retired)

BEFORE THE HEARING OF THE
SCIENCE, RESEARCH AND TECHNOLOGY SUBCOMMITTEE
ON

THE ADEQUACY, DIRECTION AND PRIORITIES FOR THE AMERICAN SCIENCE AND TECHNOLOGY EFFORT

March 1 1989

ADEQUACY, DIRECTION AND PRIORITIES----

John F. Waymouth
March 1 1989

Mr. Chairman, Honorable Members, Staff of the Subcommittee, Ladies and Gentlemen:

Thank you for offering me the opportunity to present my views on this most important subject at this hearing. I would like to preface my remarks with the statement that I speak only for myself, and that my views do not necessarily represent those of my former employer, or of any other organization of which I happen to be a member. Any remarks I might make about industrial and academic science and technology are to be taken as general opinions, and are not to be construed as applying to any specific persons or organizations.

INTRODUCTION

All of us in this room have enjoyed over the last forty or fifty years the economic and political benefits of the unchallenged technological supremacy of the United States: a supremacy which we have in many areas either lost already or are in the process of losing, in spite of the expenditure of considerable sums of money, both public and private, in support of science and technology.

I believe it is fair to say, however, that the erosion of our position results more from the success of others than from failures on our part. Total funds for R&D in the US have generally increased faster than inflation over the last two decades, even if they have not expanded at the exponential rate of the fifties and sixties. In several other industrialized countries, however, their expanding economies, and an even greater commitment to technology as a basis for continuing that expansion, have resulted in their expenditures achieving parity with, even surpassing, our own.

Consequently, since we no longer dominate in resources available to commit to technology, we must become more efficient than we have been, and more efficient than our competitors are in achieving economic benefits from that expenditure of resources. Otherwise, not only will we no longer be superior, we will be inferior in technology and its economic benefits to our competitors. As the focus of this hearing demonstrates, that means that our national effort must not only be adequate, it must be selective and prioritized, and above all, it must be effective.

ADEQUACY, DIRECTION AND EFFECTIVENESS

My views on this subject are highly colored by my 38-year career in industry, working along the interface between science and technology. In my opinion, the best science in the world is useless if it doesn't eventually find its way into new or improved products, or new and improved manufacturing processes. As I am sure you are aware, many of my academic colleagues take violent exception to this point of view, regarding product and process development and engineering as tasks that are beneath them, worthy only of technicians. They fulminate verbally and in print that it is demeaning for the body politic to ask that their work have some ultimate economic benefit in exchange for continuing support.

For their part, the technologists whose job it is to convert scientific results into useful articles of commerce or lower-cost higher-quality manufacturing techniques are remarkably poorly connected to the scientific information network. The process of bringing that information to the point of use is haphazard at best, as is the process of presenting industrial needs to the academic scientists. The technologists have little first hand contact with academic scientists, and regrettably tend to regard them as useless eggheads when they do. Moreover, because the function is almost universally understaffed, the technologists have little time to read (and certainly no time to study the scientific literature). Consequently, scientific information reaches the point of use by a slow diffusion process, not arriving there until it is already widely known. Because of this tribalism, I believe US industry gains little or no advantage from the acknowledged US pre-eminence in research. The transfer of scientific information into technology development is haphazard at best, and obstructed by culture clash at worst.

I believe that other countries do a more effective job in coupling the results of scientific research into the technology base than we do. In Germany, for example, it is common practice for graduate scientists to leave the university to work in industry for a decade or more, before returning to the university as faculty, to train new scientists. In the US, most university science faculty have never worked in any other environment than the cloistered halls. One need not be a student of psychology to guess that communication between the tribes will be better in the Germanic model.

The Japanese method for capturing the benefits of scientific progress is also instructive. By comparison with ours, the Japanese effort in research is negligible. However, Japanese industrial firms outman their American counterparts by two or three to one in product and process development and engineering. And their development and engineering personnel aggressively seek out the best the world has to offer in research results, which know no national boundaries. Moreover, service in the technology branch of most Japanese companies is an important aspect of the experience background of the top management. In the US, technology and manufacturing is no longer the route to the top of any major company. Law, finance, sales, personnel are.

I find it somewhat ironic that the science establishment of this country sells assiduously the concept that support of pure research is essential for our economic survival, while at the same time, the US Government is attempting to pressure the Japanese into providing **more** support of research, because they "pirate the results of others and don't contribute their fair share".

To summarize:

I believe the US support (public and private) of science is in total, adequate. A significant portion of that is wasted, however, on military research of no use whatever to the civilian economy. What is truly inadequate is the support of development and engineering to capitalize on the results of scientific discovery. I believe that this support must come from American industry, and not from the US Government. The role of Congress here can only be to change the ground rules to make it extremely profitable for industry to increase its development and engineering efforts, and extremely unprofitable for it not to do so. I have no suggestions on how to do this, since the problem is economics, not technology.

Unless science and scientists are integrated more effectively into the development and engineering process, however, effectiveness of that process will continue to suffer. In this context, the model provided by the State of Pennsylvania is one the Congress might well consider emulating. The State of Pennsylvania provides funding for university researchers participating in University-Industry partnerships aimed at improving the technological base of Pennsylvania companies. The concept of partnership and participation on the part of industry helps insure that projects will be chosen from the outset for their expected impact on technology, and that positive results will quickly be put into practice. If more of the national funding of research were tied to the partnership concept, much of the prioritizing that now concerns the Congress would happen naturally. The increased involvement of the scientific community into the technological needs of the nation's industry would enormously facilitate the transfer of technology. I note that Australia, which has excellent science (virtually all publicly supported) and little native industry is moving in this direction, to couple its scientific resources into developing an industrial base.

PRIORITIES

At present, there is not a universally-accepted method of ranking different scientific and technical endeavors, to serve as a basis for the allocation of funds. Within individual sciences or technologies, peer-review has worked well for many years, but it affords no more basis for comparisons across disciplinary boundaries than the proverbial "apples and oranges". In addition, recent successes in bypassing the peer-review process to obtain expenditures directed by Congress as budget line-items suggest that even that allocation method will cease to be effective.

The "Weinberg Criteria" have frequently been used as a guide for evaluations that cross disciplinary boundaries. In my opinion, they are valuable as far as they go, but are incomplete. The three criteria summarized in the Charter of this Hearing, are in fact one: "What is the value of success?" To that one, I would add others, as follows.

1) What is the value of success?
   a) Alternatively, what is the value of failure? (In the game of "Twenty Questions" with Nature, a "No" answer to a well-posed question is as valuable as a "Yes" one).

2) What is the probability of success? If all research programs are successful, we aren't being daring enough

3) What is the probable cost of success, and how long will it take?

Having stated the dimensions along which different programs should be valued, I wish I could provide quantitative means of computing a figure of merit. In common with the testimony of other witnesses on the subject, however, I regret that I cannot. The best I can do is to provide anecdotal rules of thumb from my own experience. Others, by virtue of different experience, will have different dimensions and different rules of thumb.

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John F. Waymouth
March 1 1989

1) The value of success is ultimately set in the marketplace. It is the most difficult to evaluate in advance, especially for research investigations whose payoff is far into the future. The argument is frequently made that unexpected byproducts and spinoffs may ultimately be more valuable than the success of the project itself. While this is a good argument for support of science in general, it is not a valid basis for choosing between project A or B, since both will produce spinoffs.

What can be said, however, is that those sciences which deal with what happens on earth are far more likely to provide results useful on earth than those which do not. Forced to select only one out of the Superconducting Super Collider, the Space Station, and the Human Genome Project, I would go unequivocally for the latter. Atomic, Molecular, and Electron Physics, and Condensed-Matter Physics are far more likely to have beneficial impact on industry than High-Energy Physics.

2) Scientists and engineers are incurable optimists, whose training somehow did not include knowledge of the "Law of Natural Perversity of the Universe" (A generalization of Murphy's Law). They invariably overestimate the probability of success.

3) A scientific or development project that requires a factor of ten improvement in some figure of merit beyond that evident at the time of discovery in order to be successful will require ten to one-hundred man-years, $1M to $10M 1988 dollars for completion, regardless of what the budget says.

Each successive factor-of-ten improvement will cost ten times as much as the previous one. This scales at least linearly: Factor ten, say $5M; factor 100, $50M; factor 1000, $500M; factor 10,000, $5B; factor 100,000, $50B, etc, etc, etc. Magnetically-confined thermonuclear fusion is a case in point. Experimentally demonstrated quite inexpensively in the fifties, with product of density-times-confinement-time one-millionth of that needed for "ignition", after 25 years and tens of billions of dollars, it has improved five factors of ten, but is still one short. Inertial fusion faces similar hurdles.

This rule of thumb is primarily useful in deciding when not to support the early stages of a program, unless the ultimate resources required can be forseen to be available. In my experience as an R&D director in industry, it meant a judgement that the first time a new technology was seen, it had to be a critical percentage as good as it ever needed to be to be commercially useful. Otherwise I killed the project, because I would not have the resources to finish it. The critical percentage, of course, depends on the resources which can be made available, in competition with other projects. The criterion can also be used as an early-warning signal against over-optimistic proposals.

SUMMARY AND CONCLUSIONS

Before more spending or prioritizing of spending on RD&E in the US can have any beneficial impact in reversing the decline of the US economy relative to those of its competitors, several things must happen.

Academic science and scientists must accept as a gospel the view that the society that supports them has a right to demand ultimate economic benefit in exchange for that support. It and they must be brought into the same kind of partnership with industry which exists in other nations.

American industry must double and triple the manning of the product and process development and engineering function, in order to accelerate the process of absorption of science into technology and into the economy. It must simultaneously improve the coupling of the development and engineering functions to the academic research community, indeed to its own research labs. Congress can help by acting to protect responsible managements looking to the future from irresponsible raiders, domestic and foreign, seeking only to maximize present profits. The technology function must once again become a valid pathway to the top of major American corporations.

Public spending in support of science must make connection to useful technology an essential factor in the equation, and not squander billions on scientific boondoggles of importance only to the participants. Scientific prestige has zero value in the balance of trade.

The sooner spending on military research and development can be scaled back, the more funds will be available to achieve useful results for industry and the economy.

CURRICULUM VITAE
JOHN F. WAYMOUTH

Date of Birth:	May 24, 1926
Education:	Bachelor of Science, University of the South, Sewanee, Tenn (1947); PhD Physics, Massachusetts Institute of Technology, Cambridge, Mass (1950).
Employment:	GTE Products Corporation and predecessor companies; 1950-1969, individual contributor and first-level supervisory positions; 1969-1988, Director of R&D for GTE Sylvania Lighting Products Group; retired May 31, 1988, presently independent consultant.
Publications:	36 Published articles, one book ***Electric Discharge Lamps,*** MIT Press (1971).
Patents:	45 Issued, 1 pending.
Field of Specialization:	Electric discharge in gases, and light source applications thereof.
Societies:	Fellow, American Physical Society; Fellow, Illuminating Engineering Society of North America.
Honors, Awards:	Elenbaas Prize, 1973.
Public Service:	Evaluation Panel, Heat Division, National Bureau of Standards, 1972-1975; Technical Electronic Product Radiation Safety Standards Committee, Bu. of Rad. Health, 1977-79; Advisory Committee for Physics, National Science Foundation, 1981-83.

Mr. **Brown**. Thank you very much, Dr. Waymouth.

Mr. Hecht, would you like to go ahead with your statement.

Mr. **Hecht**. Thank you, Mr. Chairman.

Both Dr. Keyworth and Dr. Waymouth have addressed the question of coupling of research with product development, and that's really the thrust of my remarks. I'd like to suggest some approaches to improving coupling mechanisms.

While I'm currently the Executive Director of the Iacocca Institute at Lehigh, my background is in U.S. and international manufacturing operations. I've had many years of experience dealing with many of the high-tech companies in Japan, Korea, and Europe; been responsible for the U.S. management of significant joint ventures in Japan; and had companies such as Toshiba, Matsushita, Sony, Mitsubishi, and others as significant customers of my U.S. divisions; and have been involved in significant product development efforts with these companies and with joint venture companies in Japan and Korea.

So today I'd like to comment on the economic effectiveness of U.S. science and technology, specifically by contrasting U.S. RD&E efforts with those in Japan and making suggestions based on my personal experience over many years in consumer electronics, technical glass, and other areas.

As has been said, the U.S. is still the world leader in a dollar sense in R&D. However, the structure of R&D in the way it is carried out in the U.S. is radically different than in Japan. In Japan, almost all R&D is carried out by individual corporations or groups of corporations. Government support may be provided in a direct dollar sense or through the encouragement of low-cost financing by Japanese banks. Applied R&D at the university level in Japan is, frankly, insignificant, and defense-related R&D is also very small.

By definition, therefore, R&D priorities are primarily determined by the private sector but with influence and direction in an informal sense from Government ministries. Generally, I feel that Japanese applied research is extremely effective, particularly in the area of product development and design, but basic research is not particularly good.

Japanese R&D groups tend to be organized within corporations in a multi-disciplinary group, with a vague organization structure and a great deal of flexibility for reaching the goals of the group. This approach allows flexibility in research direction, depending upon the success of a particular R&D effort. It also means that when the need for a particular development effort goes away, the group that is doing the effort literally disappears, with the engineers and scientists involved dispersing and being redeployed to other research efforts. The Japanese are extremely good at stopping R&D efforts, changing organizations, and changing directions when R&D efforts are no longer relevant.

In the U.S., R&D efforts tend to be more highly institutionalized, whether they are in university laboratories, government research facilities, or corporations. Research organizations tend to be relatively stable, to the detriment of program flexibility. Organizational boundaries tend to be severe and tend to restrict the flexibility of an individual research group to pursue appropriate goals, and, worst, the walls between university R&D, government laboratory

R&D, and corporate R&D weaken total economic effectiveness by making it difficult to focus all relevant research activities, whether basic or applied.

In order to be effective, I believe that the U.S. must find ways to break down these organizational and institutional barriers and, in an American context, achieve some of the same flexibility and focus that is achieved within the Japanese system.

In consumer electronics and technical glass businesses in Japan, Korea, and Europe, I have seen examples of great successes and significant failures in R&D activities. I have seen new businesses grow from $1 million sales to $200 million sales within five years, have personally involved in write-offs of over $100 million in failed R&D activities, and in the loss of world competitive position in consumer electronics.

Based on this, I'd like to suggest the following for the portion of U.S. R&D spending which is directly available for the support of economic goals.

In order to be effective, corporate, university, and Government research should be coordinated with emphasis on focused research and product and process development which will be significant to our manufacturing industries. I believe that research consortia with active involvement from all three of these research entities are the best route to accomplish this goal. Although it is now possible to form such consortia, I believe the legislative basis and Government support for these programs needs to be improved.

Antitrust barriers—and I have personal experience with this—are still perceived by many corporations as presenting unacceptable risks when questions of R&D consortiums, company-to-company cooperation, or joint manufacturing are considered. In an antitrust sense, I believe that it's very important that we consider the world economy, not the U.S. economy, when dealing with our industrial base. It's also important to have mechanisms for pre-clearance or antitrust exemptions if the possibility of consortia or intercompany cooperation exists.

University research centers can be a natural focal point for such consortia, but additional Government support could be of great help for universities to play this catalyst role. A mechanism should also be found to encourage Federal labs to participate actively in such consortia. I would suggest, in that case, that some type of a Government research council would be appropriate, perhaps chaired by the Science Advisor reporting to the President, joined by other appropriate staff heads, and including some significant industrial representatives to help with the focus on the U.S. economy.

Other programs within the Government structure which encourage industry, university, and Federal lab cooperation should also be continued. The Engineering Research Centers currently sponsored by NSF are an example of such efforts. I think it is important to point out that these Engineering Research Centers and the consortia which I mentioned above should be self-limiting and should, in fact, be allowed to go out of business if they prove to be ineffective or no longer appropriate.

In areas which are important to the future industrial base of the United States, I believe that research and possibly manufacturing consortia offer the best possibility to achieve the focus and flexibility which characterize the Japanese R&D establishment. The willingness of consortia of major corporations to put up their own money is, to me, the best single measure of the potential economic payoff of R&D expenditures focused in a specific direction.

In this regard, Sematech is off to a good start, and HDTV, an area where I have considerable personal experience, can be another significant area of the future. An important overall goal, however, is to involve university, corporate, and Federal R&D establishments with a strong economic focus provided by the private sector.

Thank you.

[The prepared statement of Mr. Hecht follows:]

Iacocca Institute

Lehigh University

Tower Building #111
Bethlehem, PA 18015
(215) 758-5452
(215) 758-5423, fax

Laurence W. Hecht
Executive Director

Testimony before the House Science, Research, and Technology Subcommittee on "The Adequacy, Direction, and Priorities for the American Science and Technology Effort"

Laurence W. Hecht
March 1, 1989

Thank you Congressman. While I am currently the Executive Director of the Iacocca Institute at Lehigh University, my background is in U. S. and International Manufacturing operations. As senior vice-president of Owens-Illinois, I have had many years of experience dealing with many of the high tech companies in Japan, Korea and Europe. I've been responsible for the U. S. management of significant joint ventures in Japan, had companies such as Toshiba, Matsushita, Sony, Mitsubishi and others as very significant customers of my divisions, and have been involved in significant product development efforts with these companies and joint venture companies in Japan and Korea.

There are many aspects to the question of general assessment of science and technology. Rather than address all of them, I would like to limit my comments to addressing the question of the economic effectiveness of United States Science and Technology specifically by contrasting U. S. research, development, and engineering efforts with those in Japan and making suggestions based on my personal experience over many years in consumer electronics, technical glass, and other areas.

Statistics show that the United States is still the world leader in a dollar sense in R&D. However, the structure of R&D and the way it is carried out in the U. S. are radically different than Japan. The question that I would like to address is the effectiveness of R&D spending in the U. S. as it impacts the performance of our manufacturing industries.

In Japan, almost all research and development is carried out by individual corporations or groups of corporations. Government support may be provided in a direct dollar sense or through the encouragement of low cost financing by Japanese banks. Applied research and development at the University level in Japan is insignificant and defense related R&D is also very small. By definition, therefore, R&D priorities are primarily determined by the private sector but with influence and direction in an informal sense from government ministries. Generally, I feel that Japanese applied research is extremely effective, particularly in the area of product development and design. Basic research is not particularly good, with almost all efforts based on particular visions of a few top people in major Japanese corporations. The result is that this research can be either very effective or very ineffective.

Japanese R&D groups tend to be organized within corporations in a multi-disciplinary group, with a vague organization structure and a great deal of flexibility for reaching the goals of the group. This approach allows flexibility in research direction depending upon the success of a particular R&D effort. It also means that when the need for a particular development effort goes away, the group that is doing the effort literally disappears, with the engineers and scientists involved dispersing and being re-deployed to other research efforts. The Japanese are extremely good at stopping R&D efforts, changing organizations and changing directions when R&D efforts are no longer relevant.

In the U. S., research and development efforts tend to be more highly institutionalized, whether they are in University laboratories, government research facilities or corporations. Research organizations tend to be relatively stable to the detriment of program flexibility. Organizational boundaries tend to be severe and tend to restrict the flexibility of an individual research group to pursue appropriate goals. And worst of all, the walls between university R&D, government laboratory R&D and corporate R&D weaken total economic effectiveness by making it difficult to focus all relevant research activities, whether basic or applied. In order to be effective, I believe that the U.S. must find ways to break down these organizational and institutional barriers and, in an American context, achieve some of the same flexibility and focus that is achieved within the Japanese system.

In the consumer electronics and technical glass businesses in Japan, Korea and Europe, I have seen examples of great successes and significant failures in R&D activities. I have seen new businesses grow from $1 million sales to $200 million sales within 5 years and have personally been involved in write-off's of over $100 million in failed R&D activities. Based on this I would like to suggest the following:

In order to be effective, corporate, university and government research should be coordinated with emphasis on focused research and on product and process development which will be significant to our manufacturing industries. I believe that research consortia with active involvement from all three of these research entities are the best route to accomplish this goal. Although it is now possible to form such consortia, I believe the legislative basis and government support for these programs should be improved.

1. Anti-trust barriers are still perceived by many corporations as presenting unacceptable risks when questions of R&D consortiums, company to company cooperation, or joint manufacturing are considered. In an anti-trust sense, I believe that we must consider the world economy, not the U. S. economy when dealing with our industrial base. It is also important to have some mechanisms for pre-clearance or anti-trust exemptions, if the possibility of consortia or inter-company cooperation exists.
2. University research centers can be a natural focal point for such consortia, but additional government support could be of great help for Universities to play this "catalyst" role. A mechanism should also be found to encourage federal labs to particpate actively in such consortia. I would suggest, in that case, that some type of a government research council would be appropriate, perhaps chaired by the Science Advisor reporting to the President, joined by the Undersecretary of Commerce for Technology, the NSF director, the R&D chiefs of DOE and DOD, the director of DARPA, the administrator of NASA, the head of NIST and most important, some significant industrial representatives to help with the focus on the U.S. economy.
3. Other programs within the government structure which encourage industry, university, and federal lab cooperation should also be continued. The engineering research centers

currently sponsored by NSF are an example of such efforts. I think it is important to point out that these engineering research centers and the consortia which I mentioned above should be self-limiting and should, in fact, be allowed to go out of business if they prove to be ineffective or no longer appropriate.

In areas which are important to the future industrial base of the United States, I believe that research and possibly manufacturing consortia offer the best possibility to achieve the focus and flexibility which characterize the Japanese R&D establishment. Sematech is off to a good start and HDTV, an area where I have considerable personal experience, can be another significant area for the future. An important goal, however is to involve university, corporate and federal R&D establishments with a strong economic focus provided by the private sector.

Iacocca Institute

Lehigh University

Tower Building #111
Mountaintop Campus
Bethlehem, PA 18015
(215) 758-5452

LAURENCE W. HECHT

Executive Director

Laurence W. Hecht, former senior vice president of Owens-Illinois, Inc., has been named executive director of Lehigh University's Iacocca Institute. The Institute's mission is to promote the goals of global competitiveness throughout Industry. The Institute's program board is made up of Lehigh faculty. The external advisory board is chaired by Lee A. Iacocca, and includes three members of the National Economic Commission.

Mr. Hecht's career at Owens-Illinois spanned more than two decades, including 12 years at the vice-presidential level and four years as senior vice president. Based in Toledo, Ohio, Owens-Illinois is a worldwide manufacturer of glass and plastic products. The corporation was one of the 30 industrial stocks in the Dow Jones index and a member of the Fortune 100 before being taken private in a leveraged buyout in 1987.

Most recently, Mr. Hecht served as Senior Vice-President, Consumer and Specialty Glass, with general management responsibility for three major divisions, consisting of 23 plants which employed 10,000 people in the U.S., Europe and the Far East. Mr. Hecht has substantial experience in high technology businesses, with management experience with wholly owned and joint venture companies in Japan, Korea, Australia, New Zealand, Indonesia, Mexico, Canada, England, Belgium, Germany and Italy.

Mr. Hecht has had management responsibility for all of Owens-Illinois corporate technical activities and International technical assistance programs in 30 countries. Mr. Hecht also managed all technical glass activities including the division that manufactured 2/3 of the glass envelopes for TV tubes manufactured in the United States, with affiliated companies in Japan (with 50% market share) and Korea (with 30% market share). He served as chief of staff for Owens Illinois International Operations, with over $2 billion in sales in 27 countries.

He served on the Board of Governors of the Electronic Industries Association for 8 years.

Mr. Hecht holds a bachelor of Science degree in industrial engineering from Northwestern University and a masters degree in business administration from Stanford.

Mr. BROWN. Thank you very much, Mr. Hecht.

I'll recognize Mr. Ritter first.

Mr. RITTER. Thank you, Mr. Chairman.

I want to thank our panelists for truly excellent testimony. I only wish that the TV cameras were in here blazing away, because I think the four of you have presented ideas that are really at the cutting edge of the future of this country, and I think that's part of our problem—the science and technology activity doesn't get the kind of attention that it should in the national scene.

Let me ask you to each comment on Dr. Keyworth's idea of a Department of Science and Technology or moving towards that over time, the idea of a civilian ARPA, and whether or not the present organization of the Federal R&D economy is up to the task that each of you has outlined. We can start off with Dr. Good.

Ms. GOOD. One of my problems, Congressman Ritter, is that I'm not a believer in organizational theory. The world doesn't progress by how you organize things, it progresses by the will of the people that you put in place to get things done.

Mr. RITTER. Could I interject and ask you a question right on that comment?

Ms. GOOD. Yes.

Mr. RITTER. Do you see us conducting a selection of personnel to put into place new ideas? Do you see us doing that in a way that will make a difference where there is unity amongst the players, there is a team approach? Are we in the process of doing this in this Administration? Should we be doing this?

Ms. GOOD. I certainly hope we are, because I think it is truly where the issues are going to be, because the difficulty that we have is not—you know, the organizational structure we presently have can work, and my problem with a Department of Science and Technology is that, number one, it would take too long to get it in place, and I don't think we have time to do that; I really don't think you have that kind of leisure, and it would take a long time to get it done.

I think that you can take the structure we presently have. I think you can encourage the people in those positions, and, in fact, the Congress can request that budgets come in in certain formats, and certain ways, and certain planning things; you have the right to ask that.

So I think that the issue with respect to whether you have a civilian DARPA or whether you have a Government agency called the Technology Department is truly not the issue. I think it's the will; it's truly the question of whether the Congress has the will and the country has the will to make it occur. If that's not there, you can put any organizational structure in that you like, and it simply will not improve things, and it won't make any difference, you'll just be shuffling the chairs on the Titanic.

So I think, really, that it's a question of figuring out what really needs to be done and requesting that that begins as soon as possible. So it's not a question that that's a bad idea, the Department of Science and Technology, it's not a bad idea.

I think the elevation of the Office of Science and Technology, the science advisor, I think that one is very important. I disagree with Jay on that one, because it's a visible symbol that you do believe the issues are urgent and that the President has that on his agenda, and I think that's important. It's the issue that it's an important issue and therefore the person is going to be listed to and has access so that you can get those things done.

So although I don't think that these questions of whether you have a structure that accommodates that, they're not bad ideas, I just believe that the amount of energy, and effort, and resources that you would have to put in them today to get them done is not where your effort and resources ought to be.

Mr. RITTER. Dr. Brinkman.

Dr. BRINKMAN. Well, I have a somewhat different view. I think that you have to think a minute about what this Department of Science and Technology might or might not do.

My problem in the priority setting thing is that—I don't see our structure in Washington so diffuse that it is very difficult to set any priorities in this town, and so how do you rectify that? It seems to me that the Department—a Department of Science and Technology doesn't really do that, at least given the kinds of things that people talk about putting in it—namely, putting NASA in it and the energy part of the DOE and maybe NSF or something like that. That Department is of very limited scope; it doesn't have any of the biology world in it at all—NIH or any of those things. Maybe those would be in it, but I doubt it. And so it doesn't satisfy this need for this overall general priority setting.

The other thing, it seems to me, is that there are different roles that you wouldn't ever want to pull together too closely. As I've pointed out earlier, I believe the role of the National Science Foundation, the reason it was a foundation and agency was to be the Mecca of basic research. That's what it was when it was created, and I think that's a very good goal.

This need for a civilian DARPA, it seems to me that need ought to be coupled into a different world than basic research. It needs to be coupled into some overall economic planning, not just other R&D. You know, Sematech is a remedial action because we didn't do our economic planning right. So what R&D the Federal Government may want to spend on the industrial civilian side of technology is something—

Mr. RITTER. I'm concerned that, you know, after Sematech there's HDTV, and that's remedial, and then maybe 10, 20 years down the—10 years down the pike, it'll be photonics and opto-electronics—

Dr. BRINKMAN. I hope not.

Mr. RITTER. —because AT&T is the only major, big-time investor and player in the private sector.

Dr. BRINKMAN. Well, we could talk about high-definition TV, but I think we're talking about this Department. In any case, my view is that the department—I understand the conception of the Department is that it wouldn't solve some of the fundamental problems we're trying to solve here.

Mr. RITTER. Well, Mary Good has laid out a very interesting scenario, and I think that's why the chairman talked about her going to be the Science Advisor. I think, given reality, that was a darn good scenario, and—but making it work, you see, given the fact that if those—particularly if those people are not hired by the Science Advisor or by a President looking to give the Science Advisor a team to work with, each of those people will have his or her own mission agency and all the constituencies of the past and the priorities of the past that go with it.

I agree with what you are saying, I proposed it to the White House basically, but I think it's going to take a tremendous effort to make it happen. It's not going to happen naturally.

I'm sorry—Dr. Waymouth.

Dr. WAYMOUTH. Well, I think that the example of the wartime Office of Scientific Research may be appropriate, which basically moved people from academic science into development activities needed to support the war effort. I believe it was not the organization that contributed to so much to this in establishing priorities and all that sort of thing.

I tend to agree with Dr. Good that systems work in spite of organizations rather than because of them. But what happened was that people were moved together, and I think Dr. Keyworth made the point that technology transfer is a body contact sport, and I would add to it that the most useful vehicle for transporting—for transferring technology is the container that lies between two human ears, and that the mechanism that is needed, from my point of view, is something that brings academic researchers shoulder to shoulder with mechanics and product development people and so forth, so that each can see that the other doesn't have horns, and bring about this kind of transfer.

A Department of Science and Technology possibly—

Mr. RITTER. Has the Federal R&D economy—

Dr. WAYMOUTH. Sir?

Mr. RITTER. If I could just, on that last comment, "bring them together, lock horns, body contact" implies the fact that the bodies are together or in proximity.

Dr. WAYMOUTH. Yes.

Mr. RITTER. So that they can make contact. Has the Federal R&D economy had a perverse effect in supporting these structures, be it national labs or academic science, in such a way that they didn't need industry?

Dr. WAYMOUTH. I believe that the entire structure of academic, industrial, Federally supported R&D divides things into containers. It's difficult to transfer technology across an organizational boundary, it's difficult to transfer technology across a geographical boundary, and it's almost impossible to transfer across a geographical and organizational boundary at the same time.

Mr. RITTER. Mr. Hecht was talking about the walls that have grown up between these different—

Dr. WAYMOUTH. These are very definite walls, and to the extent that a Department of Science and Technology might be able to enlist and fund perhaps sabbaticals on the part of university researchers to go into industry and sabbaticals on the part of industry development engineers to work in universities, I think that any kind of mechanism that will promote mixing this dish of separate ingredients into one stew is something that is far more likely to promote the kind of coupling between product and process development and engineering and the preeminence of the U.S. in research.

Mr. **Ritter**. It was stunning to hear Larry Hecht saying that in Japan there is almost no academic research, all of it is done in the corporations. Are we hamstrung by the fact that we have lots?

Mr. **Hecht**. Oh, I don't think there is any doubt that the segregation of our research hurts its effectiveness. You get the basic research in Government labs to a significant extent, and you have the product development in the corporate laboratories, and they have very little interrelationship, and they almost by definition have different goals defined.

But to return to the other question, I think Dr. Good's comments are very important. It seems to me that it's more important to probably have a strong Presidential Science Advisor in a high level than it is to put together a new department, and I think that certainly could have a positive effect. It could, in fact, help bring together some of these different resources that we have and maybe have a little impact on the Federal establishment which is diverse. It could make—perhaps be a focus for bringing together, whether you call it a Science Advisory Council or Federal Research Council or whatever it is, to coordinate the efforts we do have.

But I think it's absolutely imperative that we try to break down these walls, and I really think that in order to do that and get an economic focus, if we can encourage more and more research consortia using capabilities from all three of these areas, I think that's the best way to do it. And the best thing about that is that, almost by definition, if you have corporations involved at the beginning, they will be much more enthusiastic about commercializing the results of these consortia when they get finished, because they are involved in the original conceptions.

Mr. **Ritter**. Dr. Brinkman.

Dr. **Brinkman**. I would just like to say I disagree with some of the things that have been said here in the sense that, if you look at Japan, Japan has a lot of basic research in its university structure. In fact, if you look at the amount of money they're spending today on high-energy physics or any of these things that we think of as our flagship basic research things, they have a very large amount of it being spent through their universities, through the Ministry of Education.

MITI and the industrial part of Japan is almost totally separated from that. In Japan, the gulf between university people, professors, and their industrial labs and industry is much larger than it is here by an enormous amount.

Mr. **Ritter**. Is there some cause and effect going on there? Is there a cause and effect phenomenon in their success?

Dr. **Brinkman**. In fact, sometimes I think the biggest problem we had is the university attitudes about research have permeated too far into American industry, rather than the other way around, and that what really needs to be revived is ideas about efficient manufacture and product realization, which is something that the universities don't have much to do with, and I don't think they need to have much to do with, except in the educational sense.

Mr. **Ritter**. That's a big need, in the educational sense. I mean that's what universities are for.

Dr. **Brinkman**. It's very, very important—right. But as far as their research, I think doing basic research is just fine.

Mr. Ritter. Dr. Good.

Ms. Good. Yes. I would like to echo some of that.

One of the problems I think that Members of the Congress have is that you have no way of getting down in the detail of some of the things that we are discussing. There's no way for you to get down at that level and make any impact. If we don't have an infrastructure at that level that knows how to get some things done, it's not going—it's really going to be difficult to get that done by the top down, and I believe that the American enterprise is not—I don't think it's on its deathbed, as an awful lot of folks do. There's an awful lot of good, innovative things going on; there's a lot of good stuff going on; we still have some of the brightest people. We do have a problem in getting more kids into science and technology; we must get at that issue.

But I think the real—if you look at the issues that you can get at, I think the business of coordinating some mechanism—the one I presented is just one—but some method of coordinating is very good. After all, priorities are already getting set—people forget that—because when the NSF budget comes to you, there are priorities already built into it; they've already had to make some major choices as to where the money went, which projects are in that budget. You know what I am saying. We are setting priorities, not perhaps as well as we could, and we surely need to—we really need to utilize our money better; we need to leverage all these dollars against each other and be sure we're not duplicating programs and things like that.

But in terms of the—all of the issues that you hear today, the one area where we really have a problem in the United States, in my view, in the competitive issue, is the issue of how to get—if you look at what things cost, research is cheap, development is expensive, and the following piece of the development, once I have it developed and I have to go build a manufacturing plant, that's very expensive. So I can do lots of ideas on the research end; we do that. We do reasonably in development, in the front-end development, but then we get locked feet when it comes to whether I'm going to put up $50 million to build a plant. So we're talking about how you get from here to there, if you will, and let me give you some real concerns I have.

We have a real problem in the materials area, and I know, Congressman Ritter, that's an area of interest to you and also to Mr. Brown, I think, as well—Congressman Brown as well. We have a real problem in advanced materials, and the reason is, these materials today have limited utility. They are performance materials, and they are very good to solve certain problems. But the volume of them at the moment isn't very big, and how do you get a company to put up a $100 million plant when he's only going to sell $10 million worth of product a year? People don't do that.

We have a real problem in how to deal with the high technology issues early on and get the thing started so that the spin-offs and the costs come down, and all of these other things. Advanced composites is as good a place to talk about as any. How do you get a company to put the up front $100 million in equipment, and buildings, and facilities to make advance composites when the only market for those at the moment is a very tenuous one in advanced aircraft? Okay?

I was down the other day at—I went down to visit the old RCA laboratories, which are now the David Sarnoff Laboratories, down in Princeton, and going through that laboratory will make you ill, for an interesting reason. The amount of technology sitting there and the quality of the people begging you to do something with it is just incredible. I mean they have developed prototypes of things that are just enormously important.

For example, they have a prototype of a flat panel display. My company needs a flat panel display for one of our avionics systems, but for the amount that we are going to sell, we can't afford to pay $25 million for the manufacturing floor to make enough for our use.

Mr. RITTER. Is this the liquid crystal display?

Ms. GOOD. That's correct.

How do I get the group together of all of the folks in the United States who are going to need flat panel displays so that you could take that technology and move with it? The Japanese are going to eat our lunch on that one, because they will have gotten it—

Mr. RITTER. You know what I think? I think the Japanese are going to eat our lunch on the supercomposite materials, because they are going to be the ones with the patience of capital, and that is the underlying theme that I hear so many people speak about that can do the 5, the 10, the 15, the 20-year patient approach, and that is something that we are trying to get this Administration interested in, that we can talk science and technology until we're blue in the face, but if people don't have the patience to apply it over a decade or more, these incredibly rich areas will go to those who do have the patience and the patient capital.

Ms. GOOD. And that's the next generation, and if we don't figure out a way to be a part of that, we really will be lost in the high technologies of the twenty-first century.

So if I had anything to leave for the Congress to do, think about two things: How do you work it out in our economy today with the tax situations and so forth we have? How do you work it out, how do you legislate things, so that money made by increasing the wealth is worth more than money made by moving the wealth about? If you could figure out how to do that, an awful lot of the rest of this stuff would take care of itself.

Mr. RITTER. I think that was a very eloquent statement.

Mr. Chairman.

Mr. BROWN. Do you have any further questions?

Mr. RITTER. No, I don't, Mr. Chairman.

Mr. BROWN. Well, I certainly concur with what Mr. Ritter has said about the value of your testimony.

I'm going to ask you just one brief—to cover just one brief subject, and we've hinted at it here already. There's quite a bit of interest here on the Hill in whether we can use as a prototype, as an example of improving Federal-industry linkage, this subject of high-definition television. You mentioned you had some familiarity, Mr. Hecht; and, Dr. Good, from what you said about the lab there, it looks like they might have a contribution to make if we could figure out how to harness it.

But we've been having some ad hoc meetings with industry, academic, and other individuals, including Members of Congress, trying to develop a strategy or a definition of a Federal role. We haven't succeeded yet, and I'd like to ask if any of you particularly would have comments about whether there is a possibility of developing a consortia approach here? Would it work? How would we define the limits of the consortia, because, you know, we're talking—when we say "high-definition television," we're talking about a complex of things.

Ms. GOOD. Yes.

Mr. BROWN. Just what suggestions do you have in the next five or ten minutes that I could mull over for a while?

Dr. BRINKMAN. I have a simple one.

Mr. BROWN. Okay.

Dr. BRINKMAN. You might have read the paper this morning. AT&T and Zenith have formed what I guess I can call a consortia—it's a partnership at least—for high-definition TV, and Federal support of that initiative would be very much appreciated.

Mr. BROWN. I haven't read that story yet, but I will look forward to reading it. I think that's excellent.

Ms. GOOD. I would make the same comment that Federal support of this initiative, whether it's a private company like Zenith or whatever, would surely make as much sense as bailing out Chrysler, and that worked out pretty good.

Mr. BROWN. Well, the question is, how do we provide the Federal support? We can, of course, offer to help them if they've got these antitrust problems. Probably their biggest need is for low-interest patient capital, and I'm not sure we can do that. We can certainly harness some of our research efforts. We could fund a program to bring together university researchers and pay for that, because that's within our charter.

Ms. GOOD. That's a good point. For example—and surely that would be a help. The labs, of course, at Princeton, the Sarnoff labs, today are now totally left to fund their own work, as you know. They are now part of SRI, and they are selling research. So the possibility of funding where they are and moving that technology forward makes sense.

Mr. BROWN. They are part of SRI, but didn't I read that SRI had been acquired, or something of that sort?

Ms. GOOD. Not to my knowledge.

Mr. RITTER. Mr. Chairman, it's a very interesting point that here was a flagship laboratory of the inventor of television.

Ms. GOOD. That's correct.

Mr. RITTER. RCA. The Sarnoff laboratory is where it all started, and it was spun off by the RCA-GE combine, because this realm of economic endeavor was considered not profitable for an American high-tech company. I think that's, in itself, a disastrous signal on the horizon.

Mr. BROWN. I would think that the lab is close enough to both New Jersey and Pennsylvania that some of the members of this committee ought to get interested in it.

Mr. RITTER. Well, I tell you, I was there; I took the same tour you did; I saw the liquid crystal flat panel display. You know, I saw it. Three weeks later at the EIA—the Electronic Industry Association—show, there's four Japanese companies marketing hand-held flat panel liquid crystal displays right now, and this was—you know, I don't want to go into detail.

Mr. **Brown**. All right. Let's hear from Mr. Hecht.

Mr. **Hecht**. I think that the comment about the AT&T and Zenith one—I hadn't seen it before either—I think is an excellent sign, and it fits with what I said before. If some U.S. companies are willing to step up and become a part of this, I think it's very encouraging, and your comment about then Government funding and support for some of the longer-term money that can be an important—play an important role in here I think is extremely important. That's a role the Government can play to bridge the gap between the shorter-term horizons that corporations are almost forced to have now and the longer-term horizons which are key to U.S. economic success and, frankly, which the Japanese can do very, very easily, and I was involved—one of the companies that I was involved in has—in Japan, that I was on the board of, and we own 20 percent of, is a company that developed the key glass technology which made the liquid crystal displays possible in Japan. So I was involved in that seven/eight years ago. So we knew all about it, but we didn't do anything.

Mr. **Brown**. Don't you think we need a good glass company to be part of this consortium that we're talking about here?

Mr. **Hecht**. Oh, I don't think that's a bad idea, but the interesting thing is that now, of course, I don't work for Owens Illinois. The Owens Illinois glass operations which relate to television now are owned 50 percent by the Japanese—instead of a few years ago when not only wasn't there any Japanese ownership but Owens Illinois owned 20 percent of the Japanese company.

Mr. **Brown**. How times have changed.

Mr. **Hecht**. Leveraged buy-outs do strange things.

Mr. **Brown**. Do you have a contribution, Dr. Brinkman?

Dr. **Brinkman**. I just want to say one thing more about high-definition TV and, in general, telecommunications. One of the biggest issues here in high-definition TV and in telecommunications is, in both places we have a fantastic view of the future. High-definition TV is a thing we can have. In my view, a ubiquitous picture telephone is a thing we can have in the future in telecommunications.

The biggest issue is how we get to that future from where we are today. It's not even a research issue, it's a regulatory—it involves all aspects of society in some sense, and the most important aspect from a Federal point of view may be the regulatory one, in defining—helping define what is going to be the standard and what high-definition TV is going to be from a technical point of view. We know how to do it in ten different ways; we've got to define—decide what it is we're going to do, and that's going to be a very important thing.

Mr. **Ritter**. Mr. Chairman, if I might comment, I think you're going to have a lot of help from the gentleman from California, because I know what he has done over the years, and he's been saying this since I got to Congress, and he was here eight years before I got here—

Mr. **Brown**. Eighty years, did you say?

[Laughter.]

Mr. **Ritter**. Look for an omnibus high-definition television bill by, I believe, late tomorrow, coming out of some concerned parties.

Mr. **Brown**. I think that's very kind and a little bit overstating the situation.

Let me just—as a final point, we sometimes tend to overplay the importance of high-definition television in addition to misunderstanding what we're talking about. Is the complex of issues surrounding this word sufficiently important to give it, we'll say, a priority that's up at the top of the heap along with other things, possibly like superconductivity or whatever other high-technology initiatives you can think of—is it truly a high-priority issue?

Mr. **Hecht**. I believe it's extremely high priority. In my experience with some of the key Japanese companies that are involved, they may call it high-definition television, but they see this as a key factor in the long-term viability of Japan in telecommunications and computers. So it may have a television label on it, but that's just the label of today rather than their view of the future, and I think their goal, while they have not been as successful as they would like in world-wide telecommunications and computers, I think this, to them, is a way to go ahead and greatly strengthen their future role in those two key areas. So I think for that reason alone it's extremely important.

Mr. **Brown**. All right.

Anything else?

Okay. We have a time deadline to get out of here, and I was just trying to move up to that time.

We do very much appreciate your contribution. It's been stimulating and will provide considerable guidance to us as we move along on some of these issues during the year. We hope to be able to call on you for additional advice from time to time. Thank you very much, and the subcommittee will be adjourned.

[Whereupon, at 12:20 p.m., the subcommittee was adjourned.]

APPENDIX I

ROBERT A. ROE, New Jersey, CHAIRMAN

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JOHN TANNER, Tennessee

U.S. HOUSE OF REPRESENTATIVES

COMMITTEE ON SCIENCE, SPACE,
AND TECHNOLOGY

SUITE 2321 RAYBURN HOUSE OFFICE BUILDING
WASHINGTON, DC 20515
(202) 225-6371

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General Counsel
DAVID D. CLEMENT
Republican Staff Director

March 16, 1989

Mr. William D. Carey
Carnegie Corporation of New York
Suite 900
11 Dupont Circle, NW
Washington, D.C. 20036

Dear Mr. Carey:

On behalf of the Subcommittee on Science, Research and Technology, thank you for the excellent and insightful testimony that you gave during our hearing on, "The Adequacy, Direction and Priorities of the American Science and Technology Effort". There was a clear consensus among the witnesses on a number of issues, including the need for a cross-cutting analysis of the federal R&D budget, which would include thorough evaluations of large science projects, the need to increase our national competitiveness, and the need to improve science and mathematics education.

In order to strengthen the hearing record on the important issues that were raised, we would appreciate obtaining your responses to the following questions:

1. In his testimony before the Senate Budget Committee on March 16, 1988, Dr. Lewis Branscomb stated that, "the rigor with which the technical benefits from new projects are assessed seems to vary inversely with the size of the project". In order to assure that large science and technology projects, costing more than some large trigger level, are critically reviewed, should a process similar to that described by Dutton and Crow in their article in American Scientist be required by Congress prior to budget submittals from the Executive Branch?

2. There was a strong consensus among the witnesses testifying in the hearing that increasing national competitiveness should be a top, if not the top, national goal. Do you think the Federal Government should establish a process to identify and prioritize new areas of technology having the potential for large payoffs that would favorably impact our national competitiveness? How can this be done? Would a Delphi-like process involving representatives from government, industry and academe be an effective means to accomplish this task?

3. The human resources upon which our national science and technology effort is built depend, in great part, on adequate funding of little science, university infrastructure, and science education. Would you subscribe to the position that fluctuations in funding in these areas should be avoided and that in, allocating new funds in the federal science and technology budget, a top priority should be given to these areas?

Mr. William D. Carey
March 16, 1989
Page 2

We intend to include your responses to these questions in the published proceedings of the hearing. Our review of the hearing transcripts should be completed by early April. Therefore, we would greatly appreciate receiving your comments within the next few weeks.

Again, thank you for providing the Subcommittee with the benefit of your knowledge and experience.

Sincerely,

Doug Walgren

DOUG WALGREN, Chairman
Subcommittee on Science,
Research and Technology

DW/Wll

Allied Signal
Mary L. Good
Sr. Vice President
Technology

Allied-Signal Inc.
P.O. Box 1021R
Morristown, NJ 07960-1021
Telephone (201) 455-4729

JW

May 1, 1989

The Honorable Doug Walgren, Chairman
Subcommittee on Science, Research and Technology
U. S. House of Representatives
Suite 2321 Rayburn House Office Building
Washington, DC 20515

Dear Congressman Walgren:

I apologize for not responding to your March 16 letter earlier. I hope my response to your questions is still useful.

1. The question was asked if a process similar to that described by Dutton and Crow in their article in American Scientist should be required by Congress prior to budget submittals from the Executive Branch. I feel that such a process would be appropriate if the science criteria on which the review will be decided can be delineated in advance. The problem with these cross-cutting reviews on large science and technology programs is that it is difficult to make comparisons between very different types of projects. Thus, if any such review is to be established, one must put the review criteria in place before the process begins. If that could occur, then such a review could be extremely beneficial.

2. National competitiveness should indeed be the top national goal of the United States. The reason that this is necessary is that all of our other goals flow from it. If we cannot maintain a competitive position where our businesses are strong and provide appropriate jobs for our people, none of our other goals are achievable. All of our national goals are based on the need to provide an appropriate standard of living for our people and a strong defense for the maintenance of our long term national stability. Neither of these goals are possible if the infrastructure of our business and service activities are not strong. These cannot remain strong unless we are, indeed, competitive with the rest of the world. It is too late to build a fence around our borders so our only alternative is a competitive position. However, the problem of identifying and prioritizing new areas of technology is that our crystal ball is for the most part not good enough. For example, I do not believe that we would have identified either high temperature superconductors

or low temperature fusion as new areas of research and development. However, these two discoveries could very well be the base of some of our biggest efforts over the next two years. There are other areas where technologies have been clearly identified which do indeed impact the national position. One of these is high density TV. This is obviously an area where we must have a leadership, or at least competitive position, since it impacts our commercial success as well as our defensive position. A Delphi-like process could be indeed used to determine those areas of technology which have the best potentials for national impact. This would mean that the choices would be between those areas which have already been identified and where a vision of their ultimate potential can be seen. A Delphi-like process to determine areas of basic research for the most part, in my opinion, would be futile since basic research by its definition is the activity where totally new and unexpected ideas arise. The competitiveness issue hinges on our ability to seize those new ideas as they are articulated and to run with them to development and manufacturing. In any case, Delphi groups should carefully be chosen and kept relatively small.

3. The issue of human resources for science and technology is a great priority. None of our organizations would be of value unless we have the personnel to appropriately tackle the problems associated with them. Thus, I wholeheartedly agree with the position that fluctuations and funding in the areas of human resources, graduate stipends, graduate research, etc., should be avoided at all costs.

I hope that these answers are of value in your overall program for determining "The Adequacy, Direction and Priorities of the American Science and Technology Effort".

My very best wishes to you.

Sincerely,

Mary L. Good /j
Mary L. Good, Chairman
National Science Board

MLG/jr

Carnegie Corporation of New York

437 Madison Avenue, New York, N.Y. 10022 • (212) 371-3200 • FAX: (212) 754-4073

Mailing Address: 11 Dupont Circle, N.W., Suite 900, Washington, D.C. 20036
(202) 265-9080 • FAX: (202) 265-7037

27 March 1989

Honorable Doug Walgren, Chairman
Subcommittee on Science, Research and Technology
U.S. House of Representatives
Suite 2321 Rayburn House Office Building
Washington, D.C. 20515

Dear Mr. Chairman,

Your letter of March 16, 1989 poses three questions to which you have requested responses.

1. The Dutton-Crowe structure for scientific priorities. This paper in American Scientist (November-December 1988) is a constructive contribution to the priorities dilemma. Although I disagree with its opening premise that the scientific community is in grave crisis (my view being that it instead suffers from a happy delirium of choices), the authors' offer a thoughtful analytical approach which puts fair and appropriate questions to the scientific advocates of large projects.

Your question is whether Congress should mandate this process as a precondition to budget requests. My response is: test it first as to its workability and its added-value for funding decisions. Congress should not impose such a requirement across-the-board in the absence of high confidence that its benefits warrant it. A focused test, accompanied by clear terms for its evaluation, and aimed at the 1992 budget process, would elicit the plus and minus insights that your Committee as well as the scientific community will need.

2. Technological priorities for competitiveness. I strongly favor government leadership in initiating a process to identify and prioritize promising technologies, but such leadership does not mean that the government should run the process. It should convene it, support it, and share in its costs, but government is not set up to pick winners over losers, and many examples of program cancellations bear this out. If a process is contemplated, it should give the strongest weight to consensus opinions derived from industrial research and development firms, and government should first clear away any legal or other institutional constraints to inter-firm consultations on strategic opportunities for technological hot pursuit. Under no conditions should Congress build a case for a Department of Science and Technology on the argument that such a department could define and execute national

technology strategies and policies. No such ideal results have ever followed the creation of Cabinet-level departments for transportation, education, housing, or energy, and we do not need yet another demonstration of myth over substance.

3. Priority for the science base and infrastructure. While fluctuations from year to year in funding little science, education, and university facilities are inherent in the real world of government, I believe that these categories comprise the core of a responsible and consistent national investment and re-investment policy toward science and technological competitiveness. This does not mean granting budgetary immunity to the science base: money alone does not guarantee excellence in research, education, or innovation. Funding should be paired with performance, and should be earned. That said, I believe that Congress's first financial responsibility is to the nation's science and technology base, which if weakened or neglected or otherwise allowed to run down will be in no condition whatsoever to advance the sciences and innovations on which our defense and economic security now depend so centrally.

It is always a pleasure to appear before your distinguished Subcommittee, and I trust that these comments are responsive.

Respectfully,

William D. Carey

William D. Carey

HARVARD UNIVERSITY
SCIENCE, TECHNOLOGY AND PUBLIC POLICY PROGRAM

Prof. Lewis M. Branscomb
Director
(617) 495-1853

John F. Kennedy School of Government
79 John F. Kennedy Street
Cambridge, Massachusetts 02138

APR 06 1989

The Hon. Doug Walgren, Chairman
Subcommittee on Science, Research and Technology
Committee on Science, Space and Technology
U.S. House of Representatives, Rayburn Suite 2321
Washington, DC 20515

Dear Doug,

I am pleased to respond to the questions in your letter of March 16 1989:

1) I do favor a process of review of very large science and technology projects that has the following features:

a) The agency seeking approval translates the political, economic, security and/or social goals into a set of technical goals, a time for their achievement and an affordable cost. It is this technical description that should be given a technical review; the broader goals cannot be evaluated by the Dutton Crowe method, and should not be decided by other than the President and Congress.

b) The process should be open to public view, should be supported by adequate documentation and should be conducted under the authority of the Congress, by an executive agency such as OSTP, but using the institutional capabilities of an established and credible institution such as the NAS/NAE.

c) The Dutton-Crowe mechanism is invented to "permit scientific advisory committees to compare competing initiatives and reach unambiguous recommendations about priority." I also favor means for so emboldening such committees. The key requirements for getting committees to do that and do it well are: (i) convince the committee the several projects are in fact competing for the same resource and (ii) that the institution whom the committee is advising has the authority and the duty to make that hard decision.

If committees in the past have waffled on chosing between space and biology or between high energy physics and materials science, it is because there was in fact no such decision about to be made in that form. Any recommendation had only down-side risk for the funding of both projects.

The Dutton-Crowe scheme is, however, far too mechanistic for my taste. It provides an illusion of objectivity without the reality -since there is no a priori way of selecting an objective jury or assigning numerical weights to the individual criteria. However, their list of criteria is not a bad guide for a group of people who might be impaneled by the academies to listen to "testimony" about the project from both advocates and critics.

Please understand that I believe such an evaluation can only be advisory to government, and will be no stronger than the wisdom and maturity of the people evaluating the evidence and the credibility of the institution that stands behind them. PSAC, in its best days, was a pretty good tool of this.

Finally, let me note that the Dutton-Crowe piece starts out talking about how advisory committees might make better decisions, and ends talking about the production of a national science agenda. I think we need many national science agendas and it is unreasonable - even dangerous - to expect all of them to be compromised in one grand administrative process outside government. While the bulk of my testimony pleads for a more centralized and expert basis for coordinating government activities and setting priorities, I ardently believe in the virtues of decentral-ization in scientific resource allocation, wherever doing so matches choices to the prospective benefits in a closed manner.

The guiding principle for the centralized component of federal resource allocation for S&T should be to central the minimum number of decisions necessary for rational policy - not the maximum number and certainly not 100 % of the decisions, as implied by Dutton and Crowe.

2) Yes, I do. I could imagine a Delphi process being used as one tool by the agencies most responsible, but most Delphi studies feed back the fads of the day and are very poor at predicting surprises. They are perhaps a useful way to test the extent of departures from consensus on matters of judgement. In this case, it is really important to know the extent to which there is a consensus in industry on what the country needs, in technology, science, education and infrastructure, to make us more competitive.

I would put much more reliance on federal executives with identified responsibility, and I urge the committee to support strongly the new Technology Administration in the Department of Commerce. Rumor has it that Dr. Murin from Westinghouse will be named Deputy Secretary; if so that is very good news. Second, Ernie Ambler, the Acting Undersecretary for Technology, has laid the ground work for the rebuilding of an advisory committee like the old CTAB. I would urge the committee to ask the Deputy Secretary how this assessment might best be made, suggest the new advisory committee be given a major role. The project should be done is such a way as to identify appropriate roles for federal agencies for the different levels of science and technology that are identified.

3) Let me divide my answer between K-12 and higher education in science and engineering.

Higher education:
In NSF, I don't think "top priority" should be given to education if that means that all the available growth will be in the Education Directorate only. If it means that the highest priority purpose of the NSF is the building of a human resource base and a research capability in the universities within which that human resource is developed, then I answer "Yes". One of our biggest competitive problems today is the low prestige and poor training of engineers for careers in design, production and quality. A major renovation of engineering education, with federal dollars matching private ones, is called for.

A major competitive problem of the future is the progressive, seemingly irreversible weakening of science and engineering training and its attractiveness to our brightest students across the board. We must invest now to avoid serious problems in the future. A most urgent part of this is to find ways to attract more young women and more minorities into careers in science and technology. If equity doesn't motivate this, demography will.

Thus I think it is realistic for the committee to ask: what are the critical skills in the American research and engineering communities and how does the institutional structure and program resource allocation policies of the NSF aim to meet those needs? I think Bloch will have an excellent story to tell, and needs even more support for his goals in interdisciplinary science, in process and systems engineering, and in the rapid buildup of American skills in "hot" high-tech areas.

I also urge the committee to champion some changes that are badly needed to strengthen research and engineering education as a whole. For example, it is shocking that predoctoral and postdoctoral fellowships are taxable, when the number of Americans pursuing these careers is declining.

K-12

Here the problem of priority is centrally a department of education issue. I don't fault the federal effort to help handicapped students, but a disproportionate fraction of DOEd funding goes to special programs of many kinds. Congress should insist that the Department of Education declare itself the willing - even eager - handmaiden of the States in a major national effort at educational reform. To hide behind the notion that education is not the federal government's problem is only an excuse. Crime generated from drug traffic, building highways, looking after people's health are no more - and no less - constitutionally federal responsibilities than education.

The committee has a special responsibility for NSF - and I think NSF's K-12 program could have even more leverage if its leadership were more heavily engaged in the national reform effort. NSF should fund at least 30% of the federal matching contribution to the National Board for Professional Teaching Standards. NSF should support the major initiative of the National Science Teacher's Association, now in the works. It should give less emphasis to responding to unsolicited proposals from colleges and universities and work much more directly with K-12 school teachers, with state agencies and private education reform institutions dedicated to systemic change - not more of the same.

NSF's main K-12 need is not more money but more commitment and more clout. Once that commitment to supporting meaningful reform in every state - through the vehicle of science and math education - was well established, NSF could then make use of much larger sums of money to rebuild our educational system.

Sincerely,

Lewis Branscomb

cc: David Robinson,
Carnegie Commission

April 1, 1989

APR 07 1989

Carnegie Mellon

Office of the President
Carnegie Mellon University
5000 Forbes Avenue
Pittsburgh, Pennsylvania 15213-3890
412-268-2200

April 3, 1989

Dear Doug:

I am pleased to answer your questions from your letter of March 16, 1989, in the order that you raised them.

1. There is a great need to take a cost/benefit approach to some of the proposed projects. Any projects over $500 million, or perhaps $300 million, should have a more exacting analysis made of them. A variety of approaches should be used to get the benefits listed. The Dutton and Crow approach could be one of those that is tried. The results should not be viewed as exact or "written in stone." They should be regarded as another input into the process of evaluation. Others can be developed. For example, sampling a panel of people from the National Academy of Sciences or Engineering on certain projects would be worthwhile. Having a group of people scattered throughout universities that might be established as a panel for you could also help you in analyzing the benefits of projects over some trigger level.

2. A process should be established by the federal government to determine those areas of technology that have the potential for a large payoff. We have to be somewhat qualified in thinking only of national competitiveness. There is no question that we are going to continue to be competitive in a variety of areas. The main point with research is that the long run is the important ingredient. We must, for the long run, put our research funds in an adequate amount into proper areas. It is research in the long run that is going to be a major factor in determining the competitiveness of any one country. I favor using the Delphi procedure with a sample of scientists and engineers from government, from the national academies, and from universities. A process of this kind could be helpful to the committee in making its decisions. It is critical for the country in terms of resource allocation in the research area.

3. There is no question that we must continue to fund individual projects that are smaller in scope than some of the centers and some of the large projects that are anticipated. It is through individual investigators who have a burning desire to solve a particular puzzle that great science emanates; at least that has been true in the past. Therefore, I would subscribe wholeheartedly to the notion of keeping the funding from fluctuating in those areas. It is important that some given amount for individual projects or for what you have called "little science" is crucial.

   The university infrastructure is always a problem for universities and, particularly, private universities. Help is needed over time to improve university facilities. We, for example, have been looking at ways of improving our chemistry facilities and, at the same time, developing some new way of teaching chemistry that will result in an increased interest among students in chemistry. There is concern in the chemical industry over the relatively few students who are choosing to major in chemistry. Our cost estimate for improvements is approximately $8 million to renovate the four floors of laboratories in our science building. We are going to do so with our own funds and with the funds we can raise from alumni, corporations, and foundations. However, it will probably take us three to five years to raise the money and get the work started. The process is slow, and there are not many foundations or corporations giving money for bricks and mortar. Help from the government is critical in this area.

   Science education at the high school and grade school levels needs to be improved if we are going to improve the number and quality of scientists in the country. This area is an investment in the future and should receive significant priority. However, an emphasis on improving science education at the university level should also be maintained. There are a number of schools, for example, that have spent large sums of money on improving laboratory facilities for chemistry, but have not changed the way in which chemistry is taught. The result is $9 million on a laboratory and three majors in chemistry. Thus, the program for improving the university infrastructure should carry with it a requirement for improvements in teaching that will increase the number of majors in the field.

I hope that these answers will supplement my testimony and will be helpful to you.

Cordially,

Dick

Richard M. Cyert

The Honorable Douglas Walgren
Chairman
Subcommittee on Science, Research and Technology
U.S. House of Representatives
Rayburn House Office Building, Suite 2321
Washington, DC 20515

MAR 29 1989

JOHN F. WAYMOUTH
CONSULTANT
16 Bennett Rd
Marblehead, MA 01945
(617) 631 9068

LUX EST VITA

March 24, 1989

The Honorable Doug Walgren, Chairman
Subcommittee on Science, Research and Technology
U. S. House of Representatives
Suite 2321 Rayburn House Office Building
Washington D. C. 20515

Dear Chairman Walgren:

Thank you for your letter of March 16 inviting my comments with respect to several specific questions. My answers are listed below, in the order of the questions.

1) The process described by Dutton and Crow is basically an expansion and formalization of the three questions I posed in my oral and written testimony as crucial to the establishment of priorities among competing scientific or technical projects or initiatives:

> What is the value of success of the initiative ?
> (Scientific, Technological, Economic, Social)
>
> What is the probability of success?
> (Feasibility, Logistics, Infrastructure)
>
> What is the anticipated cost of success?

I certainly believe that the Congress should require answers to these questions before funding any scientific or technical programs, not only with respect to large initiatives which come before it as separate line items, but also with regard to the total scientific and technical programs of the mission-oriented agencies.

Therefore, I would support the application of such a formal process to aid the Congress in evaluating the various components of the federally-funded scientific and technical activity of the United States. I would quibble to some degree with the priorities that are apparently implied by the rank-order of questions in the Dutton and Crow listing. I would rank economic benefits (new technology into industry, new industries, new jobs, solution to pressing medical or environmental problems, etc) so far above contribution to scientific awareness and national pride that they wouldn't even land on the same page. Moreover, as an aid to putting claims of future benefits in perspective, I would also ask questions concerning what contributions had been made to the health and welfare of the nation by prior programs (say over the previous quarter-century) in the general area of science and technology of the initiative in question.

2) The Delphi process, as I understand it, addresses the question "What new scientific discoveries, technological developments, new inventions are possible within the time frame in question?", and only then proceeds to the question "Are these of any use?".

I would attack the problem from the opposite end. "What are the most pressing health, environmental, technological, etc, problems facing the nation? What deficiencies exist in the present armentarium of goods available; what new products are wanted or needed?". Having answered those questions, I would then ask, "Do we have the scientific and technical information base with which to address these issues? What do we need to know that we don't know?" The answers to these questions will I believe result naturally in a prioritizing of different areas of science and technology in terms of their foreseeable future value.

I would then allocate 80% of the total dollars available for the advancement of science and technology to the support of these identified and prioritized areas. The balance should be expended in support of basic science solely for the sake of advancing human knowledge, and exploring new scientific domains for which there is no presently-foreseeable need.

The allocation of a specific fraction of the total funds to basic science for which there is no immediately perceived customer will be analogous to the "discretionary funds" available to the R&D Director of an industrial research laboratory. A Delphi process could reasonably be used to identify those areas of science with the greatest opportunity of successful achievement in selecting the most fruitful areas to support. The allocation of the large majority (80%) of the funds to R&D directed to information applicable to identified and prioritized national needs will insure that these needs are met in a timely manner.

Such a formalization of the direction, prioritizing and funding of R&D would only be effective if it dealt with not only the entire Federal support of science and technology, but also at least could be coordinated with the science and technology expenditures by industry.

Given the parochial, fragmented nature of the present process of Federal support of science and technology by multiple agencies and Cabinet Departments jealously guarding their turfs, not to mention the predeliction of Congress to earmark line item expenditures within agency budgets for specific purposes, I have grave doubts that such a system could ever be instituted. It would be very useful, however, to engage in the exercise at least once to learn how far the results of the present method deviate from what we would do under a more rational allocation system (or conversely, perhaps how close they come!).

3) I certainly subscribe to the idea that multi-year funding commitments are needed for many academic scientific projects. It was a painful experience of my service on the Advisory Committee for Physics of the National Science Foundation to realize that second and third year grants to existing projects were being reduced below the originally-approved budgets because of shortfalls in the budget of the Foundation itself. This is extremely disruptive to the projects and to the organizations involved.

A major positive result of the Federal support of academic science is the output of trained scientists, who are in turn employed in scientific research and development. Without these people, it doesn't matter how much money is spent for science; there isn't anybody to do the research. The cost of training scientists in "tabletop science" is far less than in "big science" because of the much lower first and operational costs of the experimental apparatus involved. Consequently, I agree that "small science" should be given high priority on the basis of its favorable cost-effectiveness along this dimension.

With regard to infrastructure, I was dumbfounded to learn during my service on the NBS Advisory Committee for Physics of the obsolescence and shortage of laboratory apparatus in much of the nation's university system. It was particularly upsetting to me that universities which were skimming 30-40c out of every grant and contract dollar for overhead and administrative costs were apparently reinvesting little if any of that money in updating laboratory apparatus. I would deal with the university infrastructure problem by making the allowance for overhead in grants and contracts contingent upon reinvestment of at least a certain fraction of it in laboratory facilities and apparatus. A portion of the overhead in the budget of an industrial R&D lab is specifically allocated for this purpose, as is the provision of a capital expense budget for major items.

Thank you for the opportunity to express my views on these subjects. If I may be of any further service to the Subcommittee, please advise.

Very truly yours,

John F Waymouth

John F. Waymouth

Iacocca Institute

Lehigh University

Tower Building #111
Bethlehem, PA 18015
(215) 758-5452
(215) 758-5423, fax

Laurence W. Hecht
Executive Director

March 31, 1989

Congressman Douglas Walgren
Chairman
Sub-committee on Science, Research
and Technology
U.S. House of Representatives
Suite 2321 Rayburn Office Bldg.
Washington, DC 20515

Dear Congressman Walgren:

In response to your letter of March 16, I certainly concur with the need for a cross cutting analysis for the federal R&D budget to include thorough evaluation of large science projects. I also agree with the two priorities of increasing national competitiveness and improving Science and Mathematics education.

With respect to point one of your letter, it could certainly be useful to follow a similar process to that mentioned in the Dutton and Crowe article. This type of evaluation process could certainly give guidance to Congress although there would be obvious political problems in arriving at the criteria used for evaluation and in choosing the panel which would actually make the evaluation. In that regard, however, if the goals are to increase our national competitiveness and improve Science and Mathematics education, then the particular criteria espoused by Dutton and Crowe are no longer fully appropriate. They suggest three criteria, first scientific merit, second, social benefits and third, programatic concerns.

The two principal criteria suggested at your sub-committee hearings would both fall within the social benefits category, but be radically different than the specific listing in the Dutton and Crowe article. I would suggest that a consideration of scientific merit, social benefits, and programatic concerns is still appropriate, however, the two primary criteria under social benefits be the two suggested in the sub-committee hearing, that is: the prospect that a particular project could increase national competitiveness and whether the particular project could improve science and mathematics education. This again relates specifically to your point two in the March 16 letter.

As I testified during the sub-committee hearing, I think it is critical for government, industry and academe to be involved in technological activities in areas that are important for national competitiveness. For these groups to be actively involved in research and technical activities they should certainly be in on the prioritizing and selection of these activities. If industrial representatives do not become part of the selection process for these activities, it is unlikely they will pick them up and commercialize them and thus it is also unlikely that the net effect on national competitiveness of such a project would be significantly positive. As a matter of fact, the opposite could be true. A major technological effort could end up contributing to the technology base of other nations where corporations might be encouraged or coerced to pick up the results of U.S. technology programs. It is then, important that the selection and prioritizing process for new areas of technology, deeply involve representatives of industry, government laboratories and academe. Only in this way can you expect some degree of enthusiastic participation and follow-through from these groups. A delphi-like process is certainly one possibility for the accomplishment of this participation.

Point three of your letter deals with the question of consistency of funding for science and technology efforts. I certainly agree that there needs to be a consistency of funding levels in this type of activity. Erratic funding levels discourage talented individuals from devoting their life's efforts to important areas of technology. Unfortunately however, the relative inflexibility of research and technology efforts within some government, academic and corporate laboratories can easily mean that level funding does not guarantee effective application of funds. I believe that it is often necessary and appropriate to change priorities significantly within certain types of science and technology funding over time. This flexibility, is to me, the most difficult part of the process.

Based upon my experience in Japan, I feel that it is in this area where Japanese efforts are often more productive than American efforts. A technical or development effort which is no longer appropriate is often terminated in Japan without any perceived threat or difficulty to the individuals involved in that effort. They are instead, re-deployed to other efforts which have by that time received higher priority within a corporation or a government laboratory. Thus, while fluctuation in funding should be avoided in the U.S. system, changes in funding priorities and termination of projects which are no longer timely, needs to be made easier than at present.

I hope that these comments are useful to the sub-committee in their deliberations.

Sincerely,

APPENDIX II

Table 1
BIG SCIENCE FACILITIES: COSTS OF OPERATIONS AND DOING RESEARCH AS A PERCENTAGE OF TOTAL FEDERAL RESEARCH FUNDING, BY CATEGOR

Category of Big Science Facilities	Funding for Operations and Research a/ 1986	1987 ($ millions)	1988	Federal Research Funding 1986	1987 ($ millions)	1988	Facilities Funding as a Percentage of Federal Research Funding 1986	1987 (percent)	1988
1. High-Energy & Nuclear Physics	515.8	550.4	607.7	618 d/	653 e/	706 e/	83.5	84.3	86.1
2. High-Energy Physics	(374.8)	(376.2)	(408.4)						
3. Nuclear Physics	(141.0)	(174.2)	(199.3)						
4. Magnetic-Confinement Fusion	149.3	140.4	132.2	321 d/	315 e/	298 e/	46.5	44.6	44.4
5. Inertial-Confinement Fusion	66.6	54.5	68.1	147.4 f/	151.6 f/	159.0 f/	45.2	35.9	42.8
6. Materials Science and Energy	80.4	100.0	145.4	1,982.9 g/	2,071.3 g/	2,107.8 g/	4.1	4.8	6.9
7. Astronomical	45.1	46.0	46.5	468.2 h/	545.7 h/	530.1 h/	9.6	8.4	8.8
8. Atmospheric & Oceanographic b/	---	---	---	---	---	---	---	---	---
9. Space	160.3	166.2	156.4	1,448 d/	1,473 e/	1,545 e/	11.1	11.3	10.1
10. Aeronautical	99.1	124.0	115.2	775.6 i/	813.3 i/	881.5 i/	12.8	15.2	13.1
11. Supercomputers c/	150.0	178.1	181.8	302.4 j/	281.4 j/	298.0 j/	49.6	63.3	61.0
12. Engineering (nuclear reactor)	151.0	134.4	113.7	833 d/	770 e/	839 e/	18.1	17.5	13.6

Caveat: Because the categories of accumulated "Federal Research Funding" obtained from several data summary sources and set forth in table 1 may not be congruent with the categories of "Funding for Operations and Research" obtained from agency sources during the data collection phase of this study, the estimates of "Facilities Funding as a Percentage of Federal Research Funding" should be considered only as approximate even though the numbers are calculated to the first decimal place.

a/ Data obtained from tables 2 through 12.

b/ Data for the Federal Oceanographic Fleet currently are not available; see table 8.

c/ Two supercomputer facilities are included in other categories; see table 11.

d/ U.S. National Science Foundation. Federal R&D Funding by Budget Function: Fiscal Years 1986-88. (NSF87-305). Washington, U.S. Govt. Print. Off., Mar. 1987. p. 25, 37, 48, 50.

e/ U.S. National Science Foundation. Federal R&D Funding by Budget Function: Fiscal Years 1987-89. (NSF88-315). Washington, U.S. Govt. Print. Off., Apr. 1988. p. 27, 42, 54, 56, and 58.

f/ U.S. Dept. of Energy. Congressional Budget Request: Atomic Energy Defense Activities, Volume 1, FY1988. p. 39; and Budget Highlights: FY1989. p. 21.

g/ U.S. Dept. of Energy. Federal Funds for Research and Development: Fiscal Years 1986, 1987, and 1988. (NSF87-314). Washington, U.S. National Science Foundation, 1987. p. 247. (Hereinafter cited as "Federal Funds . . . ") Data for "chemistry, physics and Physical Science nec" minus data for high-energy and nuclear physics from table 1.

h/ U.S. National Science Foundation. Federal Funds for Research and Development: Fiscal Years, 1986, 1987, and 1988. Astronomy. p. 247.

i/ Ibid., Engineering, Aeronautical.

j/ Ibid., Computer Sciences.

Table 2
HIGH-ENERGY PHYSICS FACILITIES

Organization	Facility	Principal Funding Agency	Funding for Operations and Research 1986	1987	1988
			($ millions)		
Cornell University	Electron Storage Ring (CESR)	NSF a/	6.8	7.0	7.3
Fermilab		DOE	170.3	177.9	190.4
	Proton Synchrotron				
	Energy Saver-Superconducting Proton Synchrotron				
	Tevatron I - Antiproton/Proton Colliding Beam Facility				
	Tevatron II - 1,000 GeV Fixed Target Research Facilities				
	Collider Detector at Fermilab (CDF)		(9.0)	(3.2)	(2.8)
	D-Zero Detector at Fermilab		(7.0)	(10.5)	(11.4)
Stanford Linear Accelerator Center (SLAC)		DOE	116.9	110.7 b/	121.2
	SLAC Linear Accelerator				
	Positron Election Project (PEP)				
	Stanford Linear Collider (SLC)				
	Stanford Linear Detector (SLD)		(5.9)	(12.7)	(13.4)
Brookhaven National Laboratory (BNL)		DOE			
	Alternating Gradient Synchrotron (AGS)		80.8	80.6	89.5
	Accumulator Booster		(1.9)	(2.5)	(8.3)
Total			374.8	376.2	408.4

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a/ Partial data: NSF does not routinely keep data on research by the performers at this facility.
b/ In FY1986, this figure includes $2.6 million for the Mark II Detector.

Table 3
NUCLEAR PHYSICS FACILITIES

Organization	Facility	Principal Funding Agency	Funding for Operations and Research 1986	1987	1988
			($ millions)		
Michigan State University, National Superconducting Cyclotron Laboratory (NSCL)	Coupled Superconducting Cyclotrons	NSF a/	4.4	5.1	5.5
Indiana University Cyclotron Facility (IUCF)		NSF a/	5.6	6.0	6.5
Brookhaven National Laboratory (BNL)	Tandem/AGS Heavy Ion Facility	DOE	9.9	15.2	16.4
Argonne Tandem/Linac Accelerator System (ATLAS)		DOE	9.2	9.8	10.5
Lawrence Berkeley Laboratory (LBL)		DOE	32.7	34.1	34.8
	Bevalac		(27.7)	(29.0)	(29.6)
	88-Inch Cyclotron		(5.0)	(5.1)	(5.2)
Los Alamos National Laboratory (LANL)	L.A. Meson Physics Facility (LAMPF)	DOE	48.4	51.6	51.9
Oak Ridge National Laboratory (ORNL)	Holifield Heavy Ion Research Facility	DOE	10.2	9.9	9.8
MIT	Bates Linear Accelerator Center	DOE	9.3	10.9	11.7
Continuous Electron Beam Accelerator Facility (CEBAF) b/		DOE	4.9	23.8	43.7
University-Based Accelerators	(Yale, Washington, Texas A&M, Duke)	DOE	6.4	7.8	8.5
Total			141.0	174.2	199.3

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a/ Partial data: NSF does not routinely keep data on research by the performers at this facility.
b/ The projected annual operating costs upon completion are $32 million (FY1987 dollars).

Table 4
MAGNETIC-CONFINEMENT FUSION FACILITIES

Organization	Facility	Principal Funding Agency	Funding for Operations and Research 1986 ($ millions)	1987	1988
Princeton Plasma Physics Laboratory (PPPL)		DOE	79.3	83.3	83.6
	Princeton Large Torus (PLT)				
	Princeton Beta Experiment (PBX)				
	Tokamak Fusion Test Reactor (TFTR)				
GA Technologies, Inc.,	Doublet III-D	DOE	28.8	29.2	29.6
MIT	Alcator C	DOE	8.6	a/	a/
Lawrence Livermore National Laboratory (LLNL)		DOE	22.0	2.0	--0--
	Tandem Mirror Experiment Upgrade (TMX-U)		--0--	--0--	--0--
	Mirror Fusion Test Facility (MFTF-B) b/				
Oak Ridge National Laboratory (ORNL)		DOE	10.6	25.9	19.0
	Advanced Toroidal Facility (ATF)		a/	(18.4)	(17.8)
	International Fusion Superconducting Magnetic Test Facility (IFSMTF)		(10.6)	(7.5)	(1.2)
Total			149.3	140.4	132.2

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a/ Temporarily shut down for upgrades.
b/ Facility never operated.

Table 5
INERTIAL-CONFINEMENT FUSION FACILITIES

Organization	Facility	Principal Funding Agency	Funding for Operations and Research 1986	1987	1988
			($ millions)		
Lawrence Livermore National Laboratory (LLNL)					
	High-Energy Laser Facility ("NOVA")	DOE	41.0	28.7	36.7
Sandia National Laboratories (SNL)		DOE	24.1	24.4	29.7
	Particle Beam Fusion Accelerator II (PBFA II)				
	Electron Beam Fusion Accelerator		a/	a/	a/
University of Rochester, Laboratory for Laser Energetics					
	National Laser User Facility ("OMEGA")	DOE	1.5	1.4	1.7
Los Alamos National Laboratory (LANL)		DOE	b/	b/	b/
	High-Energy Laser Facility ("ANTARES")				
Total			66.6	54.5	68.1

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a/ No longer operational; technology had progressed to PBFA I and then to PBFA II prior to FY1986.
b/ No longer operational.

Table 6
MATERIALS SCIENCE AND ENGINEERING FACILITIES

Organization	Facility	Principal Funding Agency	Funding for Operations and Research 1986 ($ millions)	1987	1988
National Institute of Standards and Technology		DOC	6.4	10.4	13.4
	NIST (NBS) Research Reactor (NBSR)		(6.4)	(5.9)	(6.9)
	Cold Neutron Research Facility		—0—	(4.5)	(6.5)
MIT	Francis Bitter National Magnet Laboratory (NML)	NSF a/	4.1	3.5	3.8
University of Wisconsin		NSF a/			
	Wisconsin Synchrotron Radiation Center (SRC)		1.7	2.5	3.0
	Tantalus, Aladdin				
Brookhaven National Laboratory (BNL)		DOE	33.2	33.5	32.8
	High Flux Beam Reactor (HFBR)		(8.5)	(10.9)	(11.2)
	National Synchrotron Light Source (NSLS)		(24.8)	(22.6)	(21.6)
Stanford Linear Accelerator Center (SLAC)		DOE			
	Stanford Synchrotron Radiation Laboratory		10.5	11.8	13.5
Los Alamos National Laboratory (LANL)		DOE			
	Neutron Scattering Center (LANSCE)		—0—	6.3	11.5
Oak Ridge National Laboratory (ORNL)		DOE	14.5	15.8	23.2
	High Flux Isotope Reactor (HFIR)				
	Oak Ridge Research Ridge (ORR)				
Cornell University Electron Storage Ring (CESR)					
	Cornell High Energy Synchrotron Source (CHESS)	NSF a/	1.0	1.0	1.3
Argonne National Laboratory (ANL)		DOE	4.9	7.0	15.2
	Intense Pulsed Neutron Source		(4.9)	(5.0)	(5.2)
	6-7 GeV Advanced Photon Source		—0—	(2.0)	(10.0)
University of Missouri Research Reactor (MURR)			0.3	0.4	0.5
MIT	Research Reactor		0.2	0.2	0.2
Lawrence Berkeley Laboratory		DOE			
	1-2 GeV Synchrotron		—0—	3.5	18.6
Sandia National Laboratories		DOE			
	Combustion Research Facility (CRF)		3.6	4.1	8.4
Total			80.4	100.0	145.4

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a/ Partial data: NSF does not routinely keep data on research by the performers at this facility.

Table 7
ASTRONOMICAL FACILITIES

Organization	Facility	Principal Funding Agency	Funding for Operations and Research 1986	1987	1988
			($ millions)		
California Institute of Technology					
	Palomar Observatory				
	200-Inch Telescope		a/	a/	a/
National Astronomy and Ionosphere Center (NAIC)		NSF b/	5.7	5.9	5.9
	Arecibo Observatory				
	1,000-Foot Radio/Radar Telescope				
National Optical Astronomy Observatory (NOAO)		NSF b/	22.7	23.1	23.3
	Kitt Peak National Observatory (KPNO)				
	Four-Meter Telescope				
	Cerro Tolo Inter-American Observatory (CTIO)				
	CTIO Four-Meter Telescope				
National Radio Astronomy Observatory (NRAO)		NSF b/	16.7	17.0	17.3
	140-Foot Radio Telescope				
	NRAO Very Large Array (VLA)				
Total			45.1	46.0	46.5

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a/ Palomar does not routinely provide this type of data.
b/ Partial data: NSF does not routinely keep data on research by the performers at this facility.

Table 8
ATMOSPHERIC AND OCEANOGRAPHIC FACILITIES

			Funding for Operations and Research		
Organization	Facility	Principal Funding Agency	1986	1987 ($ millions)	1988
National Center for Atmospheric Research (NCAR)		NSF	36.1	36.1	36.1
Woods Hole Oceanographic Institution	Deep Submergence Research Vessel (DSRV) "ALVIN" and Supporting Facility	NSF, ONR, OTHER	9.2	8.5	10.4
Federal Oceanographic Fleet			a/	a/	a/
	Navy				
	NOAA				
	CG Icebreakers				
	USGS				
	EPA				
	NSF	NSF	(20.9)	(21.7)	(23.1)
	University-National Oceanographic Laboratory System (UNOLS)	NSF part	(31.8)	(35.0)	(35.1)
Total			b/	b/	b/

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a/ Data for the Federal Oceanographic Fleet currently are being collected on a comprehensive and consistent basis from the several agencies involved. The data may be available in the 1987 annual report of the Federal Oceanographic Fleet Coordinating Committee, due in mid-1989.

b/ Total not available; see footnote a/.

Table 9
SPACE FACILITIES

Organization/Facility	Principal Funding Agency	Funding for Operations and Research					
		1986	1987	1988 ($ millions)	(1989)	(1990)	(1991)
Physics and Astronomy							
International Ultraviolet Explorer (IVE)	NASA	6.2	6.9	4.4			
Solar Maximum Mission (SMM)	NASA	6.3	8.5	9.0			
Hubble Space Telescope (HST)	NASA	84.5	92.0	91.6		(launch 2/90)	
operations		(55.8)	(47.0)	(50.8)			
maintenance/refurbishment		(28.7)	(45.0)	(40.8)			
Cosmic Background Explorer (COBE)	NASA	---	---	---	3.4	(launch 5/89)	
Gamma Ray Observatory (GRO)	NASA	---	---	---	1.0	(launch 1-3/90)	
Extreme Ultraviolet Explorer (EUVE)	NASA	---	---	---	---	--- (launch 8.91)	0.8
Lunar and Planetary Probes							
Pioneer 10 and 11	NASA	2.5	3.1	3.5			
Voyager I and II	NASA	30.8	28.9	25.6			
Galileo (Jupiter Orbiter/Probe)	NASA	---	---	---		39.2 (launch 10/89)	---
Magellan	NASA	---	---	---	17.1	(launch 4/89)	
Mars Observer	NASA	---	---	---		(launch 1992)	
Earth Science							
Solar-Terrestrial Explorers (4 Explorers) (also see SMM separately)	NASA	17.5	13.5	10.9			
Earth Radiation Budget Experiment (ERBE)	NASA	7.6	8.3	7.0			
Upper Atmosphere Research Satellite (UARS)	NASA	0.6	0.6	0.6		(launch fall 1991)	
High-Energy Astronomy Observations (HEAO)	NASA	4.3	4.4	3.8		(data analysis only)	
Ulysses (with ESA) (originally Int'l Solar-Polar Mission)	NASA	---	---	---	---	(launch 10/90) ---	7.5
Total		160.3	166.2	156.4			

Table 10
AERONAUTICAL FACILITIES

Organization	Facility	Principal Funding Agency	Funding for Operations and Research 1986	1987	1988
			($ millions)		
National Transonic Facility (Langley)		NASA	6.6	6.8	6.9
Unitary Plan Wind Tunnel (Langley)		NASA	2.0	2.2	2.3
16-Foot Transonic Wind Tunnel (Langley)		NASA	1.4	1.6	1.7
Transonic Dynamics Tunnel (Langley)		NASA	1.9	2.2	2.2
Hypersonic Wind Tunnel Complex (Langley)		NASA	1.4	1.6	2.4 a/
Eight-Foot High-Temperature Hypersonic Wind Tunnel (Langley)		NASA	1.5	1.5	1.4
Eight-Foot Transonic Pressure Wind Tunnel (Langley)		NASA	0.4	0.5	1.0
20-Inch Mach 6 Wind Tunnel (Langley)		NASA	0.2	0.2	0.2
Hypersonic Wind Tunnel Complex (Langley)		NASA	0.6	0.8	1.1 b/
Low-Speed Wind Tunnel Complex (Ames)		NASA	5.1	7.6	8.7
Unitary Plan Tunnel Complex (Ames)		NASA	8.3	11.7	9.6
14-Food Transonic Wind Tunnel (Ames)		NASA	0.5	1.2	0.7
6x6 Supersonic Wind Tunnel (Ames)		NASA	1.9	2.4	0.2 c/
3.5-Foot Hypersonic Wind Tunnel (Ames)		NASA	0.4	2.4	2.9
8x6 Tran/Supersonic Wind Tunnel (Lewis)		NASA	3.9	5.3	2.6
10x10 Unitary Supersonic Propulsion Wind Tunnel (Lewis)		NASA	5.4	5.8	6.0
6x9-Foot Icing Research Tunnel (IRT) (Lewis)		NASA	0.8	1.2	1.4
Aeronautical Test Range (Dryden)		NASA	11.3	13.1	11.8
Differential Maneuvering Simulator (Langley)		NASA	0.1	0.1	0.2
Six-Degree-of-Freedom Motion Simulator (Ames)		NASA	0.2	0.2	d/
Flight Simulator for Advanced Aircraft (Ames)		NASA	e/	e/	e/
Vertical Motion Simulator (Ames)		NASA	7.1	8.4	8.6
Propulsion Systems Laboratory (Lewis)		NASA	4.4	7.2	5.2
Numerical Aerodynamics Simulator (NAS) (Ames)		NASA	34.1	40.5	39.1 f/
Total			99.1	124.0	115.2

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a/ Includes Mach 20 tunnel, hypersonic helium tunnels, a hypersonic nitrogen tunnel, and a Scramjet test facility.
b/ Includes Mach 8 variable density tunnel, 1/2 meter quiet tunnel, 20" SWT, Mach 6 high Reynolds number.
c/ Facility operated for three months in 1988.
d/ Facility is in stand-by mode.
e/ Facility is inactive.
f/ Includes development and operations costs.

Table 11
SUPERCOMPUTERS

Organization	Facility	Principal Funding Agency	Funding for Operations and Research 1986	1987	1988
			($ millions)		
National Center for Atmospheric Research (NCAR)					
	NCAR Scientific Computing Facility	NSF	(included in total NCAR, see table 8)		
Lawrence Livermore National Laboratory (LLNL)		DOE	64.6	77.0	77.8
	National Magnetic Fusion Energy Computer Center		(21.8)	(26.0)	(31.8)
	LLNL Computer Center		(42.8)	(51.0)	(46.0)
Los Alamos National Laboratory (LANL)					
	LANL Computing and Communications Division	DOE	58.6	56.9	58.1
NOAA	Environmental Research Laboratories	NOAA			
	NOAA Geophysical Fluid Dynamics Laboratory		10.3	10.6	10.1
NASA	Numerical Aerodynamics Simulator (NAS)	NASA	(included in table 10)		
National Advanced Scientific Computer Centers		NSF	8.3	22.2	24.1
	Princeton University				
	Cornell University				
	University of Illinois, Urbana-Champaign				
	University of California, San Diego				
	University of Pittsburgh				
Florida State University					
	FSU Supercomputer Computational Research Institute	DOE	8.2	11.4	11.7
Total			150.0	178.1	181.8

Table 12
ENGINEERING FACILITIES

Organization	Facility	Principal Funding Agency	Funding for Operations and Research 1986	1987	1988
			($ millions)		
Hanford Engineering Development Laboratory (HEDL)		DOE	65.1	64.3	52.9
	Fast Flux Test Facility (FFTF)				
	Fuel Materials Examination Facility (FMEF)				
Argonne National Laboratory (ANL)		DOE	45.7	44.6	43.5
	Experimental Breeder Reactor II (EBR-II)				
	Transient Reactor Test Faculty (TREAT)				
	Zero Power Plutonium Reactor (ZPRR)				
Idaho National Engineering Laboratory (INEL)		DOE			
	Loss-of-Fluid Test Facility (LOFT)		a/	--0--	--0--
Oak Ridge National Laboratory (ORNL)		DOE	3.1	3.4	4.0
	Calutrons Electromagnetic Isotope Separations Facility				
Lawrence Livermore National Laboratory (LLNL)		DOE	37.1	22.1	13.3
	Advanced Test Accelerator				
Total			151.0	134.4	113.7

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a/ Facility is shut down; $10,000 in FY1986 is for decontamination and decommissioning.